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AN ACCOUNT OF THE RETURN TO NUCLEAR WEAPONS TESTING BY THE UNITED STATES AFTER THE TEST MORATORIUM 1958-1961

UNITED STATES
DEPARTMENT OF ENERGY
NEVADA OPERATONS OFFICE

William E. Ogle

October 1985

Document Unclassified
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AN ACCOUNT OF THE RETURN TO NUCLEAR WEAPONS TESTING BY THE UNITED STATES AFTER THE TEST MORATORIUM 1958-1961 (U)

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WILLIAM E. OGLE

OCTOBER 1985

UNITED STATES
DEPARTMENT OF ENERGY
NEVADA OPERATIONS OFFICE

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~~NUCLEAR WEAPON DATA~~
~~CATEGORY SIGMA 1~~

~~CRITICAL NUCLEAR WEAPON~~
~~DESIGN INFORMATION~~
~~DOD DIRECTIVE 5210.2 APPLIES~~

~~RESTRICTED DATA~~
~~THIS DOCUMENT CONTAINS RESTRICTED DATA AS DEFINED IN THE ATOMIC ENERGY ACT OF 1954. UNAUTHORIZED DISCLOSURE SUBJECT TO ADMINISTRATIVE AND CRIMINAL SANCTIONS.~~

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~~H Milton Peek~~
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~~Manager, National Defense Science Program~~
~~Holmes & Narver, Inc.~~
~~Albuquerque, New Mexico~~

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Frontispiece:

The Kingfish event of Operation Dominic, showing the many different physical and chemical phenomena that follow a nuclear explosion at the "edge" of the sensible atmosphere.

WITHHELD UNDER
5 U.S.C. 552(b)(1),
EXEMPTION 1 - DOE

THIS PAGE WITHHELD
UNDER 5 U.S.C. 552(b)(1)
EXEMPTION 1, D.O.E.

FOREWORD

On August 22, 1958, President Eisenhower announced that the United States was ready to begin test ban negotiations on October 31, and to suspend nuclear weapons tests on that date for one year while the negotiations proceeded. The suspension might continue from year to year depending on progress in other areas. A week later Premier Khrushchev agreed to the same date for negotiations, but not to a moratorium. In fact, Soviet testing, in abeyance since March, resumed on September 20 with two very large explosions, and continued until November 3. In compliance with the President's statement, no U.S. tests were conducted after October 30. No further tests then were performed by either nation until the Soviets burst forth with an astonishing 45 shots in 65 days beginning on September 1, 1961. Of these, 14 were above a megaton, and one yielded 63 megatons -- the largest bomb ever fired by any nation. The Soviet program gave every evidence of careful and deliberate preparation.

Following the 1958 test suspension, the United States dismantled most of the complex infrastructure required for its own nuclear test programs, both in Nevada and in the Pacific. Almost three years later when President Kennedy found it essential to United States interests to resume testing in response to the Soviet testing, the experience for America's testing community was technically agonizing, operationally painful, and economically very costly. The atmospheric component of test resumption had especially high political obstacles and costs.

In this book, which was eight years in preparation, William E. Ogle has provided a detailed description of the events of that period. The book does not argue for or against nuclear testing underground or in the atmosphere. Rather, it presents a comprehensive account of the major difficulties that attended U.S. test resumption in both of those environments after a period of total cessation. At the time of this writing, the United States (along with several other nations) still conducts tests underground, but it has become clear that this activity sustains only a small fraction of the capability that would be required if the national interest again made it necessary to conduct tests in the atmosphere.

Dr. Ogle's book is unique in several respects. It is the only detailed account by an "insider" of United States nuclear testing. The earlier development of testing methods and weapons technology is presented as necessary background for the reader. The author, in addition to accumulating and knowledgeably screening a vast collection of original documents from the period, personally interviewed more than 70 key political, technical, and operational professionals who participated in the events described in the main part of the book. The collection of data and interviews on which this book is based will be preserved intact in the archives of the Los Alamos National Laboratory. Since many of the original sources are no longer available, this archival material is unique and irreplaceable.

Bill Ogle's professional contributions to the nation's security encompassed the full range of development, testing, and use of nuclear weapons. He played a central role in the United States nuclear test program from the first explosion at Alamogordo in 1945 through the time of his death in May 1984. During the critical periods just before and following the moratorium he served as Scientific Deputy to the Military Commanders of the Joint Task Forces that were created to carry out U.S. tests in the

Pacific. As Test Division Leader at Los Alamos, he was responsible also for a major part of the underground test program in Nevada. At the time of his death he was an active participant in deliberations at the highest levels of the Departments of Defense and Energy. A scientist, teacher, and leader with exceptional management skills, Ogle instilled in his co-workers some of his own enthusiasm and his complete dedication to the task at hand. He inspired lasting respect and affection among all of us who knew him.

Ogle was a superb communicator, whether he was dealing with the President of the United States or with a craft worker at one of the test sites. As the individual with primary responsibility for public safety, he had an unusual ability to provoke other specialists into looking deeply and thoroughly before each test event at the range of its possible consequences. The book extensively documents the conscientious and untiring efforts made, under his guidance and using all available knowledge, to protect the safety of the public and especially of those potentially at risk in and near the testing sites.

The editorial board, which undertook to complete this study after the author's death, decided that it should be left largely to the reader to determine the relevance of this account to future U.S. actions. Underground testing has continued since the ratification of the 1963 Limited Test Ban Treaty, which prohibits testing in all other environments. The Soviet Union recently has proposed, and claims to be observing, a 5-month moratorium on underground tests; the Administration has rejected the Soviet proposal. At the moment it seems very unlikely that the United States will unilaterally initiate testing in the atmosphere, but the outlook is clouded by active missile-defense programs on both sides. The history presented here shows that the Soviets are capable of secret preparations for elaborate tests, while in this respect the U.S. is severely self-constrained.

Were a need to arise in the future as suddenly occurred in September 1961, the account contained in this book should be an invaluable asset to those called upon to respond. To ignore this history may well be to repeat it. As President Kennedy said in 1961: "The Soviet Union prepared to test while we were at the table negotiating with them. If they fooled us once, it is their fault, and if they fool us twice, it is our fault." Bill Ogle has left a vital record that deserves the attention of those who may in the future be responsible for the nation's security.

John S. Foster, Jr.
September 1985

PREFACE

Note to the Reader:

At the time of his death in May 1984, William E. Ogle--known to friends and colleagues across the nation as "Bill" or simply as "Ogle"--had worked for almost eight years on this historic account. Soon after his death, the four men whose names appear at the end of this preface--all long-term friends and associates of the author--offered their assistance to consolidate and publish this unfinished work.

As the institutional sponsor of the project, and with the concurrence of, and a generous offer of assistance by, the Director of the Los Alamos National Laboratory, I commissioned these four as an editorial board, charged with early completion of a manuscript which would preserve the integrity of Dr. Ogle's work and be a useful reference for those to whom his message was addressed.

With this publication I believe they have accomplished that task.

Las Vegas, Nevada
July 1985

Thomas R. Clark
Nevada Operations Office
U.S. Department of Energy

It was not without some trepidation that we approached the task of editing and publishing this volume. Starting with the Prologue, the material is presented in decreasing order of its state of completion at the time of the author's death. Westervelt and Peek had worked with him extensively from 1979 until his death, had assisted with writing, had reviewed most of the manuscript, and had provided detailed comments and suggestions. Brownlee and Ray had read much of the draft material and had given their suggestions also. All believed that the author was reasonably satisfied with the Prologue and Chapters I and II. Chapters III and IV were not so far advanced and presented the need for significant writing effort affecting both organization and content. Ogle had intended a Chapter V, with a working title of "Lessons Learned," but we found not even an outline of that chapter. In fact, it was not clear whether these were to be lessons learned by the author or by the nation (or perhaps by the reader?). Thus, we have chosen to retitle that element the Epilogue, and we accept full responsibility for it. The Index is ours also, although our task here was essentially mechanical--organizing and cataloging the results of an enormous amount of digging and collecting by the author.

We welcomed the constraint imposed by Tom Clark, regarding the integrity of Ogle's effort. It was a condition which we ourselves imposed from the outset. Each of us was at times tempted to "improve" Ogle's draft, but for the most part we have successfully resisted that temptation. When we have become aware of errors of fact we have corrected them, but we have avoided second-guessing the author's judgment. We offer these disclaimers along with our hope that the reader who knew and worked with Bill Ogle will find that most of what follows is presented in a familiar style.

As for our own credentials, all of us were associated professionally and personally with William Ogle for many years in a variety of circumstances and relationships. To each of us he was at times a mentor. Each of us had a specialized role in one or more aspects of the history that is told here. Individually and collectively, we believe that we have been faithful to both the facts and the author's purpose.

Although Bill Ogle personally was a participant in most of the pertinent actions of the era upon which he reports, he was a disciplined writer and, therefore, a disciplined researcher. He mined the libraries and files of scores of offices and organizations and assembled a comprehensive and unique collection of official and authoritative papers. He interviewed at least 72 individuals, some several times, and preserved the original tape recordings of those interviews. The editorial board has recommended that a suitable classified repository be established at Los Alamos to house this collection and keep it intact and available for future researchers. Once that is done, this book should serve as a useful index and road map.

We will not presume to write the author's acknowledgments of assistance received, although we are quite certain of one name that would surely have been there. John C. Conrad, then Captain, U.S. Air Force, was detailed to work with Ogle from early 1974 through 1976. He assisted mightily with the research efforts, including notably the personal interviews. In our own behalf we wish to express appreciation to Janice Reeve Ogle for both encouragement and assistance in getting started and to the others in Ogle's Energy Systems Inc. family in Anchorage. Most of our work was done in Albuquerque, in space arranged for by Holmes & Narver, Inc. Milton Peek, who was our taskmaster throughout, had offices there and all of us appreciated the excellent working environment and hospitality. Getting the job planned, organized, and agreed upon was one thing; getting it done was another. Dave Buckner's assistance was invaluable in transcribing the original Energy Systems microcomputer floppy discs to a form usable by Holmes & Narver. Glenda Cremer Ponder was that indispensable person in any publishing venture who takes sentences, paragraphs, and pages, marginal notes and all, and turns them into a manuscript ready for the printer. Finally we wish to thank Tom Clark for authorizing and supporting the completion project.

What started out as a challenge and a duty has been indeed a labor of love. How many times, over these months, we have interrupted our deliberations to recall an Ogle mannerism, an expression, a statement of an evident truth. How many times his candor has given us pause. In a way that he truly would have enjoyed, there was a Bill Ogle presence in all our deliberations.

No one can predict when the nation may face a similar set of conditions in attempting to balance political imperatives against the harsh truths of science, or the constraint which must accompany diplomatic negotiations against the urgent need to be ready to move swiftly should negotiation fail. We--four among many who worked with William Ogle before, during, and after the test moratorium of 1958-1962--shared his view that this was an era the history of which should be preserved. His was an important contribution to making that record. For us it has been a rare privilege to help fulfill that purpose.

Albuquerque, New Mexico
September 1985

The Editorial Board
Roger Ray, Chairman
Robert Brownlee
H. Milton Peek
Donald Westervelt

INTRODUCTION

This work was commissioned by Major General Frank Camm, Director of Military Applications of the AEC, at the suggestion of Robert R. Brownlee, AEC leader of the Safeguard C (Readiness) program.

The intent was to relate the steps taken by the U.S. to return to nuclear weapons testing in late 1961 and 1962, after the three-year test moratorium of 1958 to 1961. Such a relation, in concept, would be useful to future planners were the termination of some similar hiatus (CTB, LTBT, etc.) to result in a sudden requirement to again renew or change drastically our testing procedures. Safeguard C of the LTBT requires that the U.S. maintain a "readiness to test in the prohibited enviroments." This work is intended as background reading for those who might have to carry out such a program.

However, very early on, the author concluded that a simple recounting of the steps taken in 1961 and 1962 would result in an inadequate understanding. It also seemed necessary to bring to the reader some of the background that set the stage for those actions. Thus, there is a rather long recounting of nuclear weapons testing problems, procedures, accomplishments, etc. prior to the actual meat of the work. The entire effort is broken into a Prologue, relating briefly the period of 1946 to 1958(?); Chapter I, giving the last six months of testing before the test moratorium went into effect; Chapter II, relating the period of the moratorium; Chapter III, relating the return to testing in Nevada; and Chapter IV, relating the return to testing in the Pacific. A Chapter V, giving some of the author's views on lessons learned, may or may not be produced.

This volume -- labelled Book I -- takes the reader through the moratorium. It is planned that Chapters III and IV, now in draft and on the word processor, will be issued in a year or two.*

Many people have helped in this effort. I am grateful to those many participants who subjected themselves to interviews and to those who were kind enough to read certain portions for accuracy of content, in particular John Malik, Don Westervelt, and Irv Woodward. My special thanks go to John C. Conrad who did the major work of documentation collection, and who assisted in producing much of the first draft material. Equally, I owe a great debt to Milton Peek who has patiently assisted me in editing the many versions. Lastly, I wish to thank N. E. Bradbury and A. D. Starbird for reading enough of the draft to conclude that they had no objections to its (classified) publication.

William E. Ogle
December 1983, Cabo San Lucas

*Ed. note: With the editors' decision to publish the entire work in one volume, this paragraph of the author's Introduction no longer obtains.

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AN ACCOUNT OF THE RETURN TO NUCLEAR WEAPONS TESTING BY THE UNITED STATES AFTER THE TEST MORATORIUM 1958-1961

PROLOGUE

In order to understand the steps taken immediately before the moratorium, during the moratorium, and at the end of the moratorium that affected the United States' capability to resume nuclear weapon testing, it is pertinent to review our status, from both the political and technical points of view, before the moratorium.

In the period of the moratorium, 1958 to 1961 and immediately afterwards, there was strong interaction between the testing activity details and the political situation in the United States. That interaction was perhaps not so strong in the period of 1946 to 1958, but nevertheless was occasionally noticeable.

Thus, there are several subjects that need to be discussed in this background: the political history of testing, which, of course, is largely the history of test ban activities; the tests themselves and their aims; the methods of testing; and the safety problems, specifically the question of worldwide fallout.

Negotiations

Perhaps it is best to remind ourselves first of the political history prior to early 1958. The period of 1943 through July of 1945 hardly belongs in this story since, on the part of the United States, it was largely a problem of active material procurement and device design, all conducted in great secrecy and, hence, discouraging external political problems were not particularly notable to the test organization. Furthermore, the story has been told in great detail elsewhere. For instance, the first volume of the official history of the U.S. Atomic Energy Commission (AEC), *The New World, 1939/1946*, by R. G. Hewlett and O. E. Anderson, Jr., covers these years in detail. Also, there are a variety of other books on this subject in those years such as Stephane Groueff's *Manhattan Project, the Untold Story of the Making of the Atomic Bomb*, John Purcell's *The Best-Kept Secret*, and recently, Martin Sherwin's *A World Destroyed*.

However, a few specific points should be made. The development of nuclear weapons in the United States* was hand in glove with the British and Canadians, and it is clear that even before the testing of the first nuclear weapon in July 1945, there was appreciable concern in these circles as to how this awesome weapon would be controlled. Roosevelt and Churchill had discussed the subject during 1943-1944. When

*In Britain, there were members of the French technical community involved.

Truman disclosed to Stalin at the Potsdam Conference that the United States was about to produce such a weapon, the subject was apparently not really new to Stalin. Kissinger* states:

> Against the background of the later disclosures of Soviet espionage there can be no doubt that Stalin was well aware of the impact of what he was being told. It is almost certain, in fact, that Stalin learned of the possibility of nuclear explosions well before Truman, who was not informed of the existence of our atomic energy program until he became President, in April of 1945.

Within a month after the Hiroshima detonation, the Soviet press was taking the attitude that the atomic bomb was not a decisive weapon, that it did not confer a basic advantage in warfare, and that all progressive forces should unite against its use. Stalin publicly ratified this view within a year, and maintained it up until the Soviets produced their own nuclear weapons.

The British and Canadian Prime Ministers, Clement R. Attlee and Mackenzie King, arrived in Washington, D.C. on November 10, 1945, for meetings with the President on the subject of a joint stand and communique on atomic energy matters. After several days of high-level discussions and detailed work on a joint draft, at which two of the American principals were Vannevar Bush and Ben Cohen of the State Department, the three nations agreed to a proposal and communique read by President Truman at a press conference on Thursday morning, November 15, 1945. Having decided not to disclose any information on the details of atomic energy, even as far as industrial applications go, for the present:

> The Anglo-American chiefs believed the United Nations should set up a commission to make specific proposals for (a) extending between all nations the exchange of basic scientific information for peaceful ends, (b) controlling atomic energy to the extent necessary to ensure its use only for peaceful purposes, (c) eliminating from national armaments atomic weapons and all other major weapons adaptable to mass destruction and (d) setting up safeguards to protect complying states from the hazards of violations and evasions.

The plan would proceed in stages, overseen by a commission, with each stage following upon the successful completion of the previous stage. This communique was followed within a month by a decision to solicit the Soviets' agreement on the basic guidelines and to seek their support of a joint proposal which would be brought before the United Nations for implementation. The details** of this concept were carried by Secretary of State Byrnes to Moscow for a meeting of the foreign ministers in mid-December (1945).

On January 7, 1946, less than six months after the testing of the first nuclear weapon, Secretary of State James F. Byrnes set up a Committee on Atomic Energy with Dean Acheson as Chairman. Other members of the Committee were Vannevar Bush, James B. Conant, Leslie R. Groves, and John J. McCloy. The major aim of the Committee was to consider controls and safeguards having to do with the development of atomic energy, with specific emphasis on the control of nuclear weapons. The Committee appointed a Board of Consultants including, among others, David E. Lillienthal, soon to become Chairman of the Atomic Energy Commission, and J. Robert Oppenheimer, who had been the wartime head of Project Y, later to become the Los Alamos Scientific Laboratory (LASL), the designers of the first nuclear weapons.

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*Henry A. Kissinger, *Nuclear Weapons and Foreign Policy*, Harper & Bros., New York, 1957, page 364.

**These details are covered comprehensively in the *The New World*, the AEC's history, especially Chapters 14 and 15.

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After several months of intensive study beginning in January of 1946, the Lillienthal Board of Consultants, and then the Acheson Committee, formed several conclusions and recommendations which they transmitted to the Secretary of State on March 17.

The Committee started with the statements made by the President and Prime Ministers that we now had a revolutionary weapon establishing means of destruction hitherto unknown, that there was no adequate military defense against atomic weapons, that no single nation could, in fact, have a monopoly of these weapons, and that the only complete protection for the civilized world from the destructive use of scientific knowledge lay in the prevention of war. Thus, the United States had already made a political commitment to seek, by all reasonable means, to bring about international arrangements to prevent the use of atomic energy for destructive purposes and to promote its use for the benefit of society. The Committee concluded that there was no prospect of security against atomic warfare in a system of international agreements to outlaw such weapons controlled only by *inspection* and similar policelike methods. In other words, there could not be a successful scheme of inspection in a real world. Here inspection had not only to do with nuclear weapon testing, but also with the control of the production of nuclear weapons. This was the philosophical framework in which atomic energy would be developed around the world for peaceful purposes. The Committee further concluded that if nations or their citizens carried on intrinsically dangerous activities, the chances for safeguarding the future were hopeless. "Intrinsically dangerous activities" meant the mining, production, and separation of uranium, the operation of reactors, hence producing plutonium, and the use of the product materials for the production of atomic weapons. They therefore proposed that an international agency be given sole responsibility for these dangerous activities, with individual nations giving up their sovereignty to that extent. To put it differently, nuclear energy, including weapons, should be in the hands of an international agency or there would be no hope of preventing nuclear war.

This study became the basis for the so-called "Baruch Plan" presented to the United Nations Atomic Energy Commission at their first meeting on June 14, 1946. The plan, in essence, proposed the establishment of an International Atomic Development Authority (IADA), to which all phases of the development and uses of atomic energy would be entrusted. It would own all mines and plants producing atomic fuel. It would manage these operations, it would have the exclusive capacity to carry on research on atomic weapons, and it would license nations to conduct their own atomic research. It would have the authority to inspect all declared and legal national activities to detect any illicit activities. The IADA would have the authority for sanctions against any violator. And lastly, the plan was to be put into effect by stages. The control system was to be established first and then the United States would halt the manufacture of atomic bombs, dispose of its existing bombs, and hand over to the authority its scientific and technological knowledge. The idea of sanctions was Baruch's own.* It appears that the Committee of Consultants considered this plan, minus the sanctions, as a genuine stab at a solution to the nuclear weapons problem and specifically considered international control, in some form, as the only practical system to prevent the eventual use of nuclear weapons. [redacted]

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*John W. Spanier and Joseph L. Nogee, *The Politics of Disarmament*, F. A. Praeger, Inc., New York, 1962, page 56.

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At the second meeting of the U.N. Disarmament Commission, on July 19, 1946 (less than a year after Trinity), a Soviet draft proposal was presented by Andrei Gromyko. The Soviet proposal was for:

> . . . prohibiting the production and use of atomic weapons and providing that within three months from its entry into force, all atomic weapons were to be destroyed. Violations of the conventions were considered to be a serious crime against humanity; severe penalties for violation were to be provided by domestic legislation; the agreement, of indefinite duration, was to come into force after the approval by the Security Council and ratification by the Council's permanent members; and all states, whether or not members of the United Nations, would be required to fulfill all provisions of the agreement.

Further, Gromyko proposed setting up other control measures to ensure observance. (*The United Nations and Disarmament*, page 13.) At this time, the United States had, as a reaction to popular emotion, largely dismantled its armed forces with the exception of retaining the nuclear capability, whereas the Russians still had under arms a great number of soldiers, probably somewhere between two and a half and three and a half million.

While a great deal of discussion was carried on in the United Nations on this subject in the years 1946, 1947, 1948, and 1949, the situation was actually quite static, with the United States standing behind its offer of the Baruch Plan and the Russians making variations of the proposal to ban all bombs with no control. The actual propaganda, of course, on the Russian side implied control but did not specify a way to accomplish it. The United States' position was that there should be control first, and then we would do away with the bombs. It is interesting to note that in this period of time, the United States conducted both the Crossroads and Sandstone operations in the Pacific, with essentially no notice of that fact being taken in the United Nations' debates and with essentially no feedback from those debates upon the operations, or vice versa.

Again, it should be pointed out that at that time the arguments were concerned with disarmament, specifically with nuclear disarmament, but including all disarmament, rather than the question of nuclear weapon testing alone.

(Parenthetically, up until the early 1950s, the United States had a position of keeping atomic and conventional disarmament talks separate, whereas the Soviet Union argued that they should be discussed simultaneously. However, both sides changed position in the early 1950s.)

The situation changed rather drastically in the period of 1949 to 1952. The Soviet Union fired its first nuclear fission device in 1949. The United States tested its first full-scale hydrogen bomb in 1952, and the Soviets fired their first hydrogen bomb in 1953. Thus, the United States and Britain no longer had a monopoly on nuclear weapons and the Baruch Plan was no longer as apparently generous a gesture as it had been at one time. On the other hand, the Soviets could no longer logically simply push the propaganda to do away with the bomb completely because they, too, now had a stockpile. (The British tested their first indigenous atomic bomb in 1952.)

The United Nations Atomic Energy Commission, which had been set up in 1946 with the hopes of being the forum and implementing body in which important steps towards solving the new problem posed by the atomic bomb and atomic energy in general could be handled, did not meet after July 29, 1949, and was subsequently dissolved in

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January of 1952.

The period of 1949 to 1953 was one that saw little formal action with respect to nuclear disarmament or test bans. Stalin, having developed the fission weapon, was apparently concentrating both on the production of the thermonuclear weapon and, more significantly, developing his nuclear forces, both weapons and delivery systems, in order to get out of the situation in which the United States had a strong military edge on the U.S.S.R. During this period, the Russian propaganda gradually changed from "do away with the bombs completely" to "nuclear warfare will only hurt the capitalistic system." At the same time, the United States became engaged in the Korean War, which led to a large American military buildup. We were, therefore, more interested in armament than disarmament. The United States joined Western Europe in the formation of NATO in 1949 and, hence, had to be concerned with the arming of that organization. As previously mentioned, we also elected in early 1950 to go ahead with the development of the hydrogen bomb, which was first fired full-scale in 1952. In the early 1950s, we began to equip our divisions in Europe with tactical atomic weapons to compensate for our numerical inferiority to the Red Army.

Possibilities of joint agreements or steps towards any types of disarmament took an abrupt turn in early 1950 when the Russians began their boycott of the U.N. In January, they walked out of the U.N. Atomic Energy Commission and in April, they did the same in the U.N. Commission for Conventional Armaments, when both of these Commissions refused their request to seat the Communist Chinese, just as the larger body of the U.N. had done. They were not to return to the U.N. disarmament activities until August of 1950, a couple of months after the Korean War had started. The period of lack of compromise between the Soviets and the U.S. on these nuclear and disarmament issues continued through Stalin's rule in Russia into early 1953. During this early period of the disarmament negotiation in the Cold War, the American intent was generally to establish a control system prior to agreeing to any disarmament and finally, to move to the mechanics of destroying nuclear arms. The Soviet position was the reverse with the elimination of nuclear weapons coming first followed by conventional disarmament and then a control system. The Russian-proposed control system would be tantamount to self-control.*

The year 1953 saw the conclusion of the Korean War, the death of Stalin, and a new administration in Washington. In August of 1953, a new Russian Premier, Malenkov, in announcing the detonation of their first hydrogen bomb, warned that the U.S.S.R. now had weapons of retaliation and, thus, an atomic war against the U.S.S.R. would be folly. In December, Eisenhower appealed for extraordinary measures to save mankind from the holocaust of a hydrogen war in his famous "Atoms for Peace" address to the United Nations on December 8, which he hoped would inaugurate an international program to develop peaceful uses of atomic energy, while acknowledging the impact of the emerging thermonuclear impasse. He proposed that governments involved in atomic research and development should begin to make joint contributions from their stockpiles of uranium and fissionable materials to an international atomic energy agency to be set up under the aegis of the United Nations. Several months later, in March of 1954, Malenkov admitted that a nuclear war would mean the ruin of the world civilization, as opposed to the previous Russian line that it would only mean the end of capitalism, but was obliged to repudiate the statement two months later.

It was now completely clear that the Baruch Plan had outlived its usefulness and that it had no chance of acceptance in a world in which both sides had nuclear stockpiles and means of delivery. A new position began to take form in the early

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*J. Spanier and J. Nogee, *Politics of Disarmament*, page 84.

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meetings of a newly formed subcommittee of the United Nations Disarmament Commission, whose task was to agree to a comprehensive and coordinated plan of disarmament. The subcommittee consisted of representatives of Canada, France, Russia, the U.K., and U.S. They began meeting in May of 1954 and continued meeting for several years. Right from the start, the Russian representative, Jacob Malik, challenged the previous Western position (the Baruch Plan) while substantially reiterating the former Soviet position on disarmament. In response, the U.S. representative, Morehead Patterson, soon conveyed through oral and written positions the fact that the United States was now more flexible in arriving at a modified position on these issues and would not adhere strictly to the Baruch Plan as originally set forth. This was to be the beginning of something of a thaw in such discussions between the two sides and the beginning of much greater activitity, if not progress, than had been seen in disarmament discussions since the rejection of the Baruch Plan in 1948. A British-French memorandum to the U.N. disarmament subcommittee,* in June of 1954, offered a prohibition on the use of nuclear weapons except in defense against aggression, and conversion of existing stockpiles to peaceful purposes. The plan would, in successive stages, freeze all military establishments, then reduce them by half and cut off manufacture of nuclear arms, then eliminate all conventional forces, and finally, abolish all nuclear weapons. All the stages would be supervised by an International Control System. The Russians immediately pointed out that the phrase, "except for defense against aggression," was a tremendous loophole, since all nations think of themselves as acting only in self-defense. Furthermore, the proposed controls were tantamount to an espionage system. However, they accepted the plan as a basis for discussion in September, and in May of 1955 the U.S.S.R. reversed its previous position and apparently accepted a control system prior to the complete elimination of nuclear weapons. However, they put in several hookers. One, they called for a ban on nuclear weapon testing, as part of the first phase of disarmament (at this point in time, the U.S. had conducted 66 nuclear detonations, including 6 above a megaton; the Russians had fired 14). Second, they proposed that the use of nuclear weapons, except for self-defense, be subject to the approval of the U.N. Security Council and, therefore, to the Soviet veto.** Third, it was required that the U.S. liquidate its military bases in other nations. They made another point: "There are possibilities beyond the reach of international control for evading this control and for organizing the clandestine manufacture of atomic and hydrogen weapons even if there is a formal agreement on international control." It is, perhaps, worthwhile to remember that point today.

At this time, the argument turned from disarmament to arms control and the latter parts of 1955 and 1956 were largely spent in profitless debate about the number of men that could be in the armed forces of China, United States, Soviet Union, France, and Britain. Again, tied to his proposal of March 27, 1956, Gromyko called for a ban on thermonuclear tests. Thus, in mid-1956, there was again a deadlock, the Soviet Union demanding a drastic reduction in manpower without extensive controls and the United States and Britain insisting on a limited disarmament agreement, including nuclear weapons guarded by strict controls. In July 1956, Gromyko agreed to the Western proposals for force levels (2.5 million each for U.S.S.R., U.S.A., and China, and 750,000 each for Britain and France), but stated that acceptance of these force levels was, amongst other things, contingent upon an immediate nuclear test ban. (By the end of July 1956, the U.S. had fired 87 shots,

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*Phillip Noel-Baker, *The Arms Race*, Oceana, New York, 1958.

**J. Spanier and J. Nogee, *Politics of Disarmament*, page 88.

[redacted]

5 U.S.C. 552 (b)(3) Ex. 3, D.O.E

[redacted] In 1954, the United States had tested its first emergency capability in thermonuclear weapons, and in 1956 tested devices that might fit in a reasonable stockpile, and specifically might fit missile delivery systems. The Russians first successfully dropped the hydrogen bomb from an aircraft in November 1955, whereas the United States did not accomplish that until May 1956. The Russians, on August 26, 1957, announced the completion of a successful test of the intercontinental ballistic missile.

The United States' reaction to these early Russian proposals was one of no particular interest in a ban on nuclear weapon tests, except as part of a broader agreement covering other measures of disarmament as well. After all, our military strategy depended upon nuclear weapons to counter Soviet superiority in conventional forces. However, the development of the Russian ICBM capability in 1957 apparently put more pressure on the United States to move in the disarmament field in some manner. Thus, at the meeting of the Disarmament Commission of the United Nations in London in 1957, after the Russians announced on June 14 that they would agree to the establishment of a control system, even on their own territory, to monitor an agreement for the cessation of nuclear weapons tests, the West indicated that it was willing now to consider test cessation an integral part of the initial stage of a disarmament agreement and would also agree to a temporary suspension of testing while a control system was being established. A 10-month suspension was mentioned. Slightly later, Harold Stassen, the American Representative, offered to extend the period to 12 months and suggested an extension for a second year should there be progress in relation to the cessation of production of fissionable materials for weapons purposes. At this point, the Russians still insisted that test cessation be considered as a separate measure, whereas the West was willing to consider the test cessation only as a portion of broader moves toward disarmament. At this meeting, Mr. Selwyn Lloyd reiterated a suggestion he had apparently made elsewhere, that a committee of technical experts be established within the framework of the disarmament subcommittee to consider possible methods of eliminating nuclear test explosions, and to investigate the requirements of effective supervision over an agreement to limit such explosions. Harold Stassen again made the suggestion in August. In both cases, "The U.S.S.R., however, refused to consent to technical talks unless there was first an agreement on the period and the conditions of a test cessation."*

(In March of 1954, the United States had detonated the Bravo shot of Operation Castle from which the debris was carried up and dispersed over a much larger area than was thought possible. As a result, an appreciable number of Marshallese natives and the crew of the Japanese fishing vessel, Fukuryu Maru ["Lucky Dragon"] received large fallout radiation doses. Not long after that, radioactive rain fell on Japan as a consequence of a Soviet hydrogen bomb test. These incidents, plus an increasing study of the quantity of radioactive material in the atmosphere and its possible effects, began to produce a move on the part of other nations to exert pressure on the United States and Russia to stop testing. Prime Minister Nehru, in an address to the Indian parliament on April 2, 1954, proposed a "standstill" agreement to stop testing, leaving the broader problems for later solution. In the years from 1954 to 1957, the Japanese and the Indians, particularly, pressed for a test ban on the part of the Russians, the United States, and the United Kingdom. Albert Schweitzer issued an appeal to the Norwegian Nobel Committee, broadcast in 50 countries, asking that

*Harold K. Jacobson and Eric Stein, *Diplomats, Scientists, and Politicians*, The University of Michigan Press, Ann Arbor, 1966, page 18.

public opinion demand an end to nuclear tests. That appeal was endorsed by the Pope in 1957. The International Labor Organization and Economic and Social Council of the United Nations recommended a test ban. In August of 1957, the World Council of Churches urged an international court to stop further testing. In the United States, Nobel Prize winner Linus Pauling urged, through many publications and public meetings, a cessation of testing because of the possible genetic effects.

The next step took place at the 12th session of the U.N. General Assembly in the fall of 1957. At that Assembly, the Russians initially proposed a two or three year test ban starting January 1, 1958, with an International Commission to supervise the test suspension and with control posts spread through the appropriate nations. That proposal was withdrawn in favor of an Indian resolution asking that the nuclear powers agree immediately to suspend tests and that a Commission of Experts be created to recommend an adequate control system. This resolution was favored by the Eastern bloc and opposed by the Western bloc and was rejected in each of two slightly different forms in November of 1957. Meanwhile, the General Assembly had adopted a resolution patterned after the earlier London Western proposals after a great deal of discussion and suggested alteration. That resolution included a number of disarmament measures which would occur simultaneously, among which was the immediate suspension of testing of nuclear weapons and prompt installation of effective international control, including inspection posts equipped with appropriate scientific instruments located within the territories of a number of countries, including the United States and Russia. The resolution also requested that the subcommittee of the Disarmament Commission convene as soon as possible to set up the implementation of this resolution, and take as one of their first tasks the establishment of "a group or groups of technical experts to study inspection systems for disarmament measures on which the subcommittee may reach agreement in principle." This entire resolution, adopted on November 14, was supported by the West and opposed by the East. Russia announced that they would no longer participate in the work of the Disarmament Commission or its subcommitee, stating that these bodies were composed in a one-sided fashion. Various moves were made to change the membership of the Disarmament Commission and its subcommittee, but none were satisfactory to the Russians.

In the United States, Adlai Stevenson, in 1956, had suggested that the United States might unilaterally stop testing as a first step toward obtaining an agreement with the Soviet Union. Bulganin, of the U.S.S.R., endorsed the Stevenson proposal. Various religious groups, in 1957, urged test cessation. In February of 1957, the Council of the Federation of American Scientists recommended that the Administration should seek worldwide cessation of nuclear weapons tests without making this contingent on achieving more far-reaching goals in arms limitations. Even in Congress, in 1957, there were proposals for halting tests, at least temporarily, to alleviate the problem of fallout. Surprisingly, one such proposal came from Chet Holifield, Chairman of the Special Subcommittee on Radiation of the Joint Committee on Atomic Energy. In November of 1957, Hubert Humphrey, then Chairman of the Subcommittee on Disarmament of the Senate Committee on Foreign Relations, suggested to President Eisenhower that the United States should declare its willingness to negotiate separately a ban on nuclear weapons tests for a two-year period, with the only condition being agreement on an effective inspection system, with United Nations supervision, to ensure that the ban was being scrupulously observed.* Thus, the pressures were high on Eisenhower in late 1957 to make some move on the cessation of tests.

*U.S. Congress, Senate Committee on Foreign Relations, Subcommittee on Disarmament Control and Reduction of Armaments, *Final Report*, 85th Congress, Second Section, 1958, page 34.

The "Security Resources Panel of the Office of Defense Mobilization Science Advisory Committee," or more briefly, the Gaither Committee, established by the National Security Council in April of 1957 to focus on the question of civil defense, concluded that the Soviet gross national product (GNP) was increasing at a much faster rate than ours and that the Soviets were spending about the same as the U.S. on their armed forces and heavy industry, even though our GNP was about three times that of the U.S.S.R. The U.S. had further concluded that the Soviets had a large nuclear stockpile, advanced missile technology, and a potential for launching a devastating missile attack on the U.S. by late 1959. The committee commented on the relative vulnerability of the U.S. civilian population and the U.S. nuclear offensive force (our so-called deterrent). Accompanying these gloomy conclusions were recommendations for substantial measures to implement a civil defense program and to vastly improve many aspects of our military offensive and defensive forces. The strong pessimism of the Gaither Report may have mainly reflected an impression of the Russian superiority in missiles at this time, coupled with the large devices they had tested. The U.S. also had a large nuclear capability, presumably even larger than that of the U.S.S.R., but mainly aircraft-carried. Perhaps the biggest justification for this report's tone was simply that for the first time in our history, we were clearly susceptible in our homeland to being attacked by a foreign nation.

President Eisenhower was not stampeded by the grim picture and far-reaching recommendations of the Gaither Report. Drawing on views of the overall situation from many other sources and advisors, he concluded that our overall military strength was still distinctly superior to that of the Communists, but he clearly saw the need for vigilance and careful study.

Up to this time, Eisenhower and Dulles had relied almost exclusively on AEC Chairman Lewis Strauss for guidance on the technical side of the nuclear weapons and nuclear test ban considerations. Strauss had long been a strong proponent of maintaining the U.S.'s superiority in these areas and dealing very skeptically with the Russian proposals. With the Gaither Report in hand, it is quite understandable that the President would have sought to have another strong scientific voice as an advisor to address fields other than nuclear weapons, i.e., missiles, civil defense, etc. Thus, he formed a new position on his staff, that of Special Assistant for Science and Technology. The first appointee was Dr. James R. Killian, President of MIT, who was appointed in November 1957, ostensibly as a reaction to the Soviets' Sputnik launch and the need to look at the nation's overall scientific effort. Concurrently, Eisenhower elevated the status of the Science Advisory Committee of the Office of Defense Mobilization, renamed it the President's Science Advisory Committee (PSAC), and brought it under White House auspices as an advisory group chaired by Killian.

Thus, 1958 began with a new, strong scientific voice in high government circles, with a great deal of pressure, both within the U.S. and without (from the U.N., Russians, and others) to work on a specific test ban agreement, and with strong pressures caused by the Gaither Report and Sputnik launch to take very seriously the question of the U.S. defense and technological status vis-a-vis the Russians. Eisenhower, in his autobiography, observed in restrospect that:

> It was now becoming apparent that both East and West needed a common understanding of the scientific technicalities involved before the possibilities of a comprehensive, regulated test ban could be intelligently discussed.*

*It does not seem to have been apparent to the Russians.

Consistent with the Russian trend of the last several years, and reflecting the Russian worries about proliferation, Premier Bulganin had proposed in a letter of December 10, 1957, to Eisenhower that the nuclear powers agree to stop nuclear weapon testing as of January 1, 1958, for a period of two to three years, and had stated his willingness to meet with Eisenhower and discuss this and other nuclear weapon control subjects. Operation Hardtack was well along in planning by this time. Eisenhower did not answer immediately, but did write to Nehru on December 15, 1957, that he could not agree to stop testing as an isolated step without assurance that other measures to settle the problems of limitations on nuclear weapon production, surprise attack, etc., would follow. However, he did answer directly on January 12, 1958, stating that he could not attend a summit conference on the subject without adequate preparatory work, and reiterating the earlier (Selwyn Lloyd) suggestion of a meeting of East and West technical experts to discuss the feasibility of test ban controls.

With this in mind, and with the further worry, expressed by Dulles at an NSC meeting in early January, that the U.N. would soon adopt a resolution condemning further testing, Eisenhower, at Killian's suggestion, requested that Killian appoint a special panel to consider the effect of a nuclear test ban on American and Soviet weapons development programs, and to what extent evasions of such a ban could be detected. Killian moved quickly in the appointment of what became known as the Bethe Panel, after its chairman, Hans Bethe of Cornell. Other members were Harold Brown, Herbert York, Carson Mark, Roderick Spence, Doyle Northrup, Herbert Scoville, Jr., Major General Richard Coiner, Brigadier General Alfred Starbird, Herbert Loper, and Colonel Lester Woodward. This group constituted a reasonable cross section of the intelligence community, the weapons laboratories, and the Department of Defense. The Panel was to report to the President's Science Advisory Committee. Over the next couple of months, the Panel collected and considered the available information (some to be used by the American contingent at the later conference of experts) and apparently* concluded that continued testing into the indefinite future could only close the gap between the U.S.S.R. and the U.S. in nuclear weapon technology, and that the offsetting gains to be expected by the U.S. from further testing were minimal. The PSAC considered the Bethe Panel findings and concluded that Hardtack should be finished, but that then we could risk a test ban with mutual inspection, and so recommended to Eisenhower.

The pressure to consider a test ban separately from other disarmament measures increased when Harold Stassen, who had just resigned as the President's Special Assistant for Disarmament, testified on February 28, 1958, to Senator Humphrey's Subcommittee on Disarmament that a separate test ban agreement would help break the spiral of the arms race, might lead to other steps of arms control and disarmament, and could be easily policed.

Another apparent gain in world opinion went to the Russians when, having just finished an extensive test series, they announced on March 31, 1958, that they had unilaterally discontinued the testing of nuclear weapons in the Soviet Union, and called on other nations to follow their lead. (The U.S. was just about to begin Operation Hardtack in the Pacific.) They pointed out that they would feel free to resume testing if other nations did so. Eisenhower, in a press conference on April 2, called the move a "gimmick" that should not be taken seriously, but Khrushchev, who had just taken over from Bulganin, reiterated the appeal in a personal letter to Eisenhower of April 4, 1958.

The Senate Committee on Foreign Relations queried, by mail, some 42 senior American seismologists, geophysicists, and geologists in April concerning the

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*H. Jacobson and E. Stein, *Diplomats, Scientists, and Politicians*, page 46.

problems of the detection of underground explosions. They received 31 replies. There was no concensus on the feasibility of detection of clandestine underground explosions.

Eisenhower answered Khrushchev on April 8, with essentially no change in his position, and on April 22, Khrushchev stated that it would be impossible for the experts to contribute to the problem of disarmament unless an agreement between governments had been reached. By now, apparently the Bethe Panel finding had sunk in a little deeper, and Eisenhower, without further consultation with the AEC or Department of Defense, wrote to Khrushchev on April 28, 1958, that the United States policy was changing, repeated his suggestion of a meeting of experts, but in the context of an agreement to stop testing, and added "Studies of this kind are the necessary preliminaries to putting political decisions actually into effect." That is, he proposed the technical meetings on the feasibility of monitoring a test ban as a prelude to opening political negotiations if such monitoring appeared reasonable.

Catching the U.S. unprepared, Khrushchev, on May 9, 1958, agreed to the meeting of the experts, but made his view clear that control was really no problem, that the experts' meeting was unnecessary, and that he regarded this as another move on the part of the U.S. to delay the cessation of testing. (By now, the U.S. was well into Operation Hardtack.)

Further correspondence during May 1958 established the date of July 1 for the conference to start. It would be in Geneva at Russian insistence rather than New York as proposed by Eisenhower. The U.S.S.R. insisted that agreement there would automatically commit the governments to a test cessation; the U.S. disagreed. There would be two sets of specialists, one of representatives of the U.S., U.K., France, and Canada, and the other of representatives of the U.S.S.R., Czechoslovakia, Poland, and Rumania. Thus, the U.S.S.R. managed to get the discussion separate from the U.N., and achieved parity of representation, which they felt they could not get in the United Nations.

During June 1958, there was a wild scramble to assemble all data that might be needed by the U.S. experts. An appreciable amount of information was declassified for such use. (But the AEC gave Captain John H. Morse, a Headquarters representative at the conference, the authority to declassify on the spot if necessary.)

After discussion among the four Western nations, the Western delegates were announced on June 20. Dr. James Fisk, a member of PSAC, was the Western chairman of the delegation. Other members were Robert Bacher, also a member of PSAC, E. O. Lawrence, Sir John Cockroft, Sir William Penney, Professor Yves Rocard, and Dr. Ormand Solandt. Advisors to the Western delegation included Hans Bethe (Cornell), Harold Brown (Livermore), Perry Byerly (University of California), Norman Haskel (Air Force, Cambridge), Spurgeon Keeny (Killian's office), J. Carson Mark (Los Alamos), Doyle Northrup (AFOAT-1)*, Herbert Scoville, Jr. (Consultant, PSAC), Anthony Turkevich (University of Chicago), Donald Morris (State), Ronald Spiers (State), and Thomas Larson (State). The AEC and Edward Teller were kept informed by telephone.

The Eastern panel consisted of Yevgeni K. Fedorov, Academy of Sciences of the U.S.S.R.; N. N. Semenov, Academician; I. Ye Tamm, Academician; M. A. Sadovsky, Academy of Sciences of the U.S.S.R.; O. I. Leypunsky, Professor of Physical-Mathematical Sciences; I. P. Pasechnik, Academy of Sciences, U.S.S.R.; Semen K. Tsarapkin, Collegium of the Soviet Ministry of Foreign Affairs; and other scientists from Czechoslovakia, Poland, and Rumania. Thus, the Eastern panel included a senior diplomat, Tsarapkin, a discipline not represented on the Western side.

*Air Force Office for Atomic Energy.

The Western delegation had available to it the experimental information collected in the past on the sound signals expected at great distances (or microbarographic signals) from nuclear detonations, electromagnetic phenomena, and a great deal of knowledge on the optical outputs of nuclear weapons fired at normal altitudes. Further, they were aware of the tropospheric disturbances from such detonations and AFOAT-1 had a great deal of experience in collecting airborne radioactive samples at appreciable distances as well as interpreting the resultant data. Seismic signals at teleseismic distances had been observed from high-yield detonations in the Pacific, but more pertinently from the NTS* Rainier underground shot. However, identification of underground shots was uncertain. Also, there was essentially no information on high-altitude detection, the highest detonation to this time being a small-yield shot at about 37,000 feet. There was no information available on deep space shots. Some underwater detonations had been observed. There was, however, appreciable theoretical information on all of these areas, some of which had been worked out specifically to prepare for the Bethe Panel report earlier in the year and expanded in preparation for the Experts Conference.

It is not clear what information the Russians had available to them when the conference began. Clearly, they had made observations close-in on normal atmospheric detonations but, to our knowledge, had not conducted any underground, underwater, or high-altitude detonations up to this time. They clearly had the same competence as we to carry out calculations to predict what would happen with shots in these other environments, but it is not known how many of these calculations they had already worked out when the conference began.

The British had available to them at the beginning of the conference the same information that we did, essentially as a result of our coordination with them on these subjects, and they also had some information they collected themselves from their tests in Australia and Christmas Island.

The conference convened as scheduled.** The Western side was clearly concerned with the possibilities of evasion and had discussed at home a number of those possibilities. During the initial parts of the conference, the Americans tended to present theories and data, with the Russians listening and making comments. A great deal of the debate and the discussion was between the various Western delegates. The Russians expressed apparent surprise at some of the theoretical concepts which the West wished to introduce. In fact, one could get the impression that the Russians had not seriously considered a number of the aspects of clandestine testing that we brought up and wished to discuss in detail. Even though this was not a political meeting, there was sometimes the impression that it was hard for the Soviets to see why we wanted to discuss these technical subjects at all since it was clear that one could simply stop testing and detect evasions very easily.*** The conference rather rapidly reached agreement on circumstances surrounding detection and identification of atmospheric tests, underwater tests, and high-altitude tests. In retrospect, it appears that perhaps underground testing was treated as the only serious medium for clandestine testing, neglecting altitudes above 50 kilometers, which the conferees knew they were not addressing in sufficient detail. The bulk of the discussion addressed the possibilities and situations with respect to underground test detection and some discussion of possibilities of test evasion. The system of detection

*NTS--Nevada Test Site, the later term used for the Nevada Proving Ground (NPG), established in 1951.

**On July 2, DeGaulle announced that the French would not agree to a test ban without other measures of disarmament. This does not seem to have affected the course of the discussion.

***Spurgeon Keeny--private communication.

stations eventually endorsed, it was felt, would allow detections and identification down to a few kilotons. The network of posts eventually agreed upon was a British-suggested compromise between U.S. and Soviet extremes. The detection stations would number some 160-170 land based plus about 10 on ships, with the land spacing being from 1,000-1,700 kilometers based on seismicity, and the spacing in ocean areas from 2,000-3,500 kilometers. The 110 posts on the continents would include 24 in North America, 6 in Europe, 37 in Asia, and the rest on the other continents: the other 60 land-based posts would be on 6 large and small oceanic islands.

Whereas the conferees discussed and formulated their findings on methods of detection at altitudes greater than 30-50 kilometers, the system described for test detection did not include coverage of that region.

On-site inspection was not treated in depth. Statements were made that teams could be sent to investigate a suspicious event, that perhaps 20-100 earthquakes a year would be indistinguishable from deep underground events at about 5-kilotons yield, but no clear-cut number of inspections was suggested.

The American team clearly felt the need of more data on seismic signals from underground detonations, and believed that further "proof tests" would be required. However, Chairman Fisk felt that this subject would "scare off" the Russian scientists, so it was never raised during the conference.

Nevertheless, the conference issued a communique on August 21, 1958, including the statement, "The conference reached the conclusion that it is technically feasible to set up, with certain capabilities and limitations, a workable and effective control system for the detection of violations of a possible agreement on the cessation of nuclear weapons tests."

The conclusions of the conference were published by the State Department on August 30 and stated that methods of test detection available at that time made it possible to detect and identify nuclear explosions down to somewhere between 1 kt and 5 kt underwater, underground, or in the atmosphere up to perhaps 10 km, and that detonations of the same yield would probably be detected but not always identified up to perhaps 50 km. The conference gave its findings on the methods of detecting nuclear explosions at altitudes greater than 50 km, but did not describe specific means for such detection and identification. The methods to be used for detection and identification included the collecting of samples of radioactive debris; recording of seismic, acoustic, and hydroacoustic waves; recording of electromagnetic waves; and on-site inspections of identified events which could be suspected of being nuclear explosions. They outlined a workable control system including appreciable development of equipment, operational considerations, data analysis, staffing, and control posts. The conference report mentioned several clandestine circumstances that might make detection or identification very difficult. They concluded the following:

> **However, the conference considers that, whatever the precautionary measures adopted by a violator, he could not be guaranteed against exposure, particularly if account is taken of the carrying out of inspection at the site of the suspected explosion.**

In retrospect, it seems that a major cause of uncertainty was that there was no agreement in the U.S. as to what yield constituted a "significant" test. The experts clearly recognized that they were really discussing a threshold situation, that bombs below some yield could probably be detonated without detection (although there is always a chance of getting caught). However, what that significant threshold is has not yet (1979) been agreed upon in the U.S.* Clearly, the value determines the number of detection stations required and the estimate of possible inspections required. It seems possible that the differences that showed up between East and West during the Conference of Experts, and become so serious in the next few years, could have come about by a genuine difference in judgment on this point, due to the different weapon design and deployment philosophies.

During mid-1958, a group to assist the President in consideration of these matters, eventually to be called the "Committee of Principals," had gradually come into existence. During this period, it consisted of the Secretary of State (John Foster Dulles), the Secretary of Defense (Neil H. McElroy), the Director of the CIA (Allen W. Dulles), the Chairman of the AEC (John A. McCone, who replaced Lewis Strauss in July 1958), the Special Assistant to the President for Science and Technology (James R. Killian, Jr.), an ex officio member, and the Special Affairs (Gen. Robert Cutler). Sessions were occasionally attended by George V. Allen, Director of the U. S. Information Agency.

Having some forewarning, the Committee of Principals discussed moves now necessary as a result of the conclusions of the Committee of Experts. It seemed necessary to enter test ban negotiations (perhaps to call the U.S.S.R.'s "bluff"), although McElroy and McCone felt that a treaty was not in the best interests of the U.S. Furthermore, Dulles wanted to stop testing at the beginning of negotiations, while AEC and Defense argued that this should only come about when the treaty came into force.

The result was that Eisenhower announced on August 22, 1958, that the United States was prepared to enter into test ban negotiations beginning October 31, 1958, and was prepared to suspend nuclear weapons testing for one year after the beginning of negotiations, with that suspension to be extended year by year depending upon the operation of the control and inspection system, and upon the progress in arms control measures. Limiting the moratorium to one year was apparently a sop to the AEC and DOD. The British issued an almost simultaneous statement.

Chet Holifield (Representative) immediately stated that the inclusion of the aim for further arms control was "susceptible of different interpretations" and that if it were maintained, he had "little hope for the completion of a nuclear testing agreement." His was a voice in the wilderness.

Now it was up to the Russians, and on August 29, 1958, Khrushchev came through with a signal of the future. The Russians were still "observing" their self-imposed moratorium, while the U.S. was testing vigorously. Khrushchev objected, in an interview with Pravda, that the U.S. was still avoiding an immediate discontinuance of nuclear tests, that the conditions for progress on disarmament were unrealistic since the lack of progress was the fault of the West, and that a one-year moratorium was just the time needed to prepare for another test series. Nevertheless, the next day the Russians formally agreed to begin negotiations on October 31, 1958, in Geneva.

Using the continued Western tests as an excuse, Russia resumed testing on September 20, 1958.

The purpose of the negotiations continued to be argued in September and October, with no agreement being reached. During this time, at the thirteenth General Assembly of the U.N., the Russians continued to try to separate "test ban" from "other measures of disarmament" with little success.

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*In 1978, the Department of Energy testified that it could not indefinitely certify the present stockpile without continued testing [redacted] and that the Soviets could test at that level for any foreseeable verification capability.

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In spite of these uncertainties, time went by, as time will, and the nuclear test moratorium went into effect for the United states at midnight, October 30, 1958, Pacific Standard Time.

The Devices

The developments of the gun device (Little Boy or Thin Man) and the Christy* implosion device (Fat Man) during the years 1943 through 1945 have received detailed historical comment and need not be discussed here. Suffice it to say that the problem was early-on recognized as one in which a sufficient mass of active material had to be assembled in short enough time that any neutron background present would have a low probability of starting a chain reaction and developing enough energy to prevent the assembly of the device. Thus, either fairly large masses of active material could be assembled slowly if one could be sure there was no appreciable neutron background, or smaller masses of active material could be assembled more rapidly. It was preferable to compress the material if possible, but this then had to be done in a comparatively short time if there was any appreciable neutron background. Furthermore, the criticality achieved had to be such that the nuclear reaction would then take place in a short time compared to the hydrodynamic times involved in disassembly. The pre-Trinity effort (1943, 1944, and half of 1945) devised two devices satisfying these conditions. One was the gun device, which was eventually used on Hiroshima. It was simply the linear assembly in a gun barrel, using a small amount of gun powder as a propellant, of a large mass of oralloy, that is uranium enriched in U-235 [redacted]

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In July of 1945 such a device had been constructed using a large fraction of the nation's separated U-235. There was no real question about its operation if there was no basic error in the whole philosophy of rapid fission chain reactions. That point could be tested with a spherical assembly. The gun device drop weapon weighed 8,900 pounds.

The other method, that of rapid assembly of the fuel by implosion with high explosive, first became practical in the so-called Christy device, or "Fat Man," [redacted]

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*After a concept of Robert F. Christy of Los Alamos Theoretical Division.

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While the initial concepts among the senior originating people of the Manhattan District at that time (Lawrence, Fermi, Oppenheimer, et al.) were of a fission bomb, it was recognized at the same time that a thermonuclear bomb might also be practical. However, it was clear that to start the burning of a thermonuclear bomb would require temperatures and pressures greater than could be achieved by existing techniques, and, furthermore, the appropriate cross sections and arithmetical methods were not available to make reliable calculations on the problem.* So while one path was clearly feasible in the light of the physical knowledge available at that time, the other was very questionable. The path of the fission reaction was taken. However, during that time, an appreciable amount of theoretical calculation was done on possible thermonuclear assemblies and burn systems, and an appreciable amount of laboratory work was done in the measurement of the appropriate cross sections for thermonuclear reactions.

After the massive exodus from Los Alamos, in late 1945, of the senior laboratory people and the revitalization under N.E. Bradbury, the designers and Laboratory experimenters began to follow up some of these concepts. [redacted]

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Lighter-weight devices were desired by the military in order that other planes than the B-29s could be used for delivery systems and, of course, a variation in yields would also offer more flexibility to the military. The Navy was strongly interested in devices for their specific applications.

No new concepts were tested on Crossroads (1946), since that was purely a Navy effects test series using two stockpile "Christy" devices. Sandstone, in 1948, after a gap of some three years since Trinity, saw the investigation [redacted]

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*It appears that the Germans put some effort on the thermonuclear concept, perhaps not realizing that only the fission bomb could produce the starting conditions necessary for sustained thermonuclear burn.

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Even though the Christy device had worked at Trinity, weapon design theory was quite primitive compared to the understanding we have today. Thus, some further time was spent in the years between Trinity and Sandstone developing a better theoretical basis for weapons calculations. The computer capability was very small compared to today, so the time required, even for a primitive weapon design calculation, was great.

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*Ed. note: LLL was renamed the Lawrence Livermore National Laboratory in 1980-1981.

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Weapons Test Operations

By the time of the Test Moratorium in 1958, several nations had tested nuclear weapons. The testing methods varied appreciably, both between nations, and, over that period of time, for United States tests. This section will attempt to outline, briefly, that history up to the moratorium, giving the methods of testing, why those methods were used, and what it was we were trying to accomplish.

The first nuclear weapons test in history by any nation took place in July of 1945, close to Alamogordo, New Mexico. The purpose of the test was to determine whether a spherical assembly system, developed at Los Alamos over the previous couple of years, would actually produce a significant chain reaction that would result in appreciable explosive energy. The device was the so-called Fat Man, [redacted] The uncertainty in whether the device would work or not was sufficiently great that it was felt worthwhile to use a large fraction of the nation's separated plutonium to test the device, rather than immediately using it in warfare.

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Initial estimates of the phenomena to be expected led the Laboratory to fire the device on a tower in order to reduce the fallout and to allow somewhat better observation of the visible phenomena than would have been feasible had it been fired directly on the ground. Estimates had been made by members of the Laboratory as to the phenomena to be expected, that is, blast pressure as a function of distance, light intensities, gamma-ray intensities, neutron intensities, etc. Trinity was the

*It is the author's memory that at the last Staff Member Meeting before the Trinity shot the various senior members of the Laboratory gave their estimates as to what the yield might be expected to be. The highest number the author remembers hearing was 7 kilotons.

most highly instrumented shot for output phenomenology that the United States was to fire for a large number of years. In restrospect, the measurements having to do with the effects of the detonation were probably appreciably more complete and advanced than those measurements having to do with the detailed working of the device itself. Presumably, this shot could have been fired as an airdrop, but obviously, the detailed measurements required would not have been feasible. The device went at some 20 kilotons, much to the pleasure of the designers. The effects, in particular fallout, were sufficiently noticeable to show that testing in that manner, in a region as highly populated as southern New Mexico, should be regarded with a jaundiced eye in the future.

It is perhaps worthy of note that early plans called for detonation of the Trinity device in a large, cylindrical container, called Jumbo. If the shot were to fail, it was supposed that this would allow recovery of the plutonium. However, after realization of the small likelihood of containment, and the difficulty of recovering the plutonium, which would be mixed with all the other bomb debris, the plan was abandoned. In a sense then, the first test planned by the United States would have been a contained clandestine test in the event of failure.

The Hiroshima and Nagasaki detonations (August 5 and 9, 1945, Greenwich time, respectively) hardly fit into the category of this history since they were not tests but were wartime attacks on a foreign nation. However, from the academic point of view of methods of testing, it is notable that these were airdrops and that there was a small amount of primitive instrumentation to determine that at least the weapons performed. The B-29 drop aircraft was accompanied to the target by two other B-29s, one with some instrumentation on board and the other to take photographs. The Los Alamos scientists did not feel it necessary to test the gun weapon used at Hiroshima (Little Boy) before its combat use since the Trinity test of the implosion device (Fat Man) offered some guarantee of the correctness of the calculations and the detonation mechanism was more predictable. In essence, if the Fat Man went all right (as it did at Trinity), the Little Boy was bound to.

The next United States nuclear weapons tests, Crossroads, in 1946, were really not tests of the nuclear weapons, but tests of the effects of nuclear detonations on ships, specifically on ships in harbors. Hence, the site chosen was chosen for the effects purposes and had nothing to do with weapons diagnostics. Crossroads saw the detonation of two more Christy devices, essentially identical to the Trinity and Nagasaki shots. The weapons diagnostics were therefore designed to show any differences between the Trinity shot and the Crossroads shot. On Crossroads Able fireball measurements were made from land-based cameras, which presumably would allow comparison of the fireball expansion with the Trinity shot. A measurement of neutron flux as a function of distance was made in order to compare with similar measurements at Trinity. On Crossroads Baker only a measurement of the high explosive transit time was made. Radiochemical analysis was made of the debris on both shots.

The first peace time airdrop of a nuclear weapon was Crossroads Able at Bikini Atoll. The measurements suffered from two problems. First of all, the timing of the detonation vs. the measurement timing was off by a number of seconds and caused certain data to be lost and, perhaps more importantly, the detonation took place about 700 yards from the planned zero point which caused certain instrumentation to be mislocated as to field of view and distance from burst. However, the variety and quantity of instruments made these problems not so serious as they might have been. The second Crossroads test, Baker, a detonation 90 feet below the water surface, provided much useful data on an underwater burst and its effects on various types of ships at varying distances. The shots generally showed that Navy vessels were quite resistant to nuclear blast, but the danger from the radioactive water was demonstrated to be quite impressive and was a serious problem to ships.

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Trinity tower cab. Fat Man device with N. E. Bradbury facing camera.

Thus, the first new nuclear weapon device test following Trinity came with the Sandstone series at Eniwetok* in 1948. The long period of time from Trinity to Sandstone probably reflected the uncertainty in this country as to the wisdom of further weapon development and certainly reflected the trauma at Los Alamos following the war, during which a large proportion of the senior staff left the Laboratory.

At that time, 1947-1948, there was not yet serious consideration of a permanent proving ground, so Eniwetok was picked on a one-shot basis for Sandstone. It was still not thought feasible to fire nuclear weapons in the continental United States and the Crossroads operation had made the testing organization familiar with atoll operation. Thus, since Sandstone was intended to be largely a ship-based operation, Eniwetok was chosen because of the comparatively good transportation through Kwajalein, the fact that it already had an airstrip and a number of facilities that could be used, and was under the control of the United States. It was necessary to remove

*Ed. note: The currently favored spelling is Enewetak, but the editors have elected to preserve the author's spelling, which was the officially recognized spelling during the years of atmospheric testing.

the Eniwetok natives. At that time, the natives had great faith in the wisdom of the United States representatives and, furthermore, had no recourse except to accede to the appropriate suggestions. (We had already moved the Bikini natives off in order to conduct the Crossroads operation.)

The use of short towers, a la Trinity, allowed similar diagnostic measurements to be made; the methods of diagnostics were now better understood and were advanced over those of Trinity. (See section on Diagnostics.) In addition to the measurements made by the test organization to diagnose the performance of the devices as well as making certain measurements of the outputs of the devices, there were effects measurements made mainly by military project teams. (See section on Effects Experiments.)

Operation Sandstone led to the concept of the development of Eniwetok as a proving ground, and was the serious beginning of the education of the crews of people who would then conduct fireball measurements, radiochemical measurements, and reaction rate measurements over the next ten years. As such, it began the training of those people who eventually established our readiness capability during the moratorium. Sandstone was, however, a simple operation from the point of view of the diagnostics required, the weapon principles being tested, and the operational problems involved, as compared with later operations. The shots were sufficiently small that the fallout was no serious problem except locally; however, the yields were large enough (49, 18, 36 kt) that firing those shots in the continental United States was thought at that time to be unwise.

Operation Greenhouse, in the early part of 1951, was, with the possible exception of Trinity, the most far-reaching and complex diagnostic operation in the period before the moratorium. In retrospect, it was probably the low computer capability as compared with today and perhaps the lack of the sense of urgency nationally that led to the comparatively long period between Sandstone and Greenhouse. At that time, at Los Alamos, there was a general feeling that a series of a few shots every two years probably adequately matched the design and postshot analysis capability of the Laboratory. Furthermore, the design and construction of the diagnostic systems, especially for George shot, was very time consuming.

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However, in late 1950, after the planning and construction and procurement were well along for Operation Greenhouse, the need for accurate yield predictions for Greenhouse led to the decision to launch a series of nuclear experiments on the

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design of the fission weapons to be used on Greenhouse, since that design now involved theory that went far beyond any past experimentation and, thus, there was some serious question as to the accuracy of the calculations.

Thus, it became necessary to mount a "quick and dirty" operation, called Ranger, to test these principles, [redacted] There was no way in the short period of time available to construct the proper facilities overseas to test these devices in the manner that had been become normal, that is, firing on short towers with fairly complex instrumentation. Since the device to be tested was in the stockpile stage with respect to high explosive and there was now enough experience in diagnostic techniques, it became clear an airdrop operation could produce the diagnostic results necessary. There was also now enough understanding of fallout to be able to predict that if the devices were fired at sufficient altitude, and the yield kept low enough, the fallout would be at sufficiently low levels that the operations could be safely conducted in the continental United States. A quick survey of possible sites in the United States led to the choice of a portion of the Air Force Gunnery Range, northwest of Las Vegas, known as Frenchman Flat. In short order, a zero point was chosen at Frenchman Flat and an alpha station designed and constructed. Alpha, the exponential rate of growth of the nuclear reaction, was measured using ion chambers on the ground close to the alpha station. The airdrop target was a cross of lights placed appropriately within the alpha detector array to allow the best coverage. Fireball cameras for yield measurements were placed at a quickly constructed control point, some seven miles away from the zero point, and on a nearby hilltop. Radiochemical

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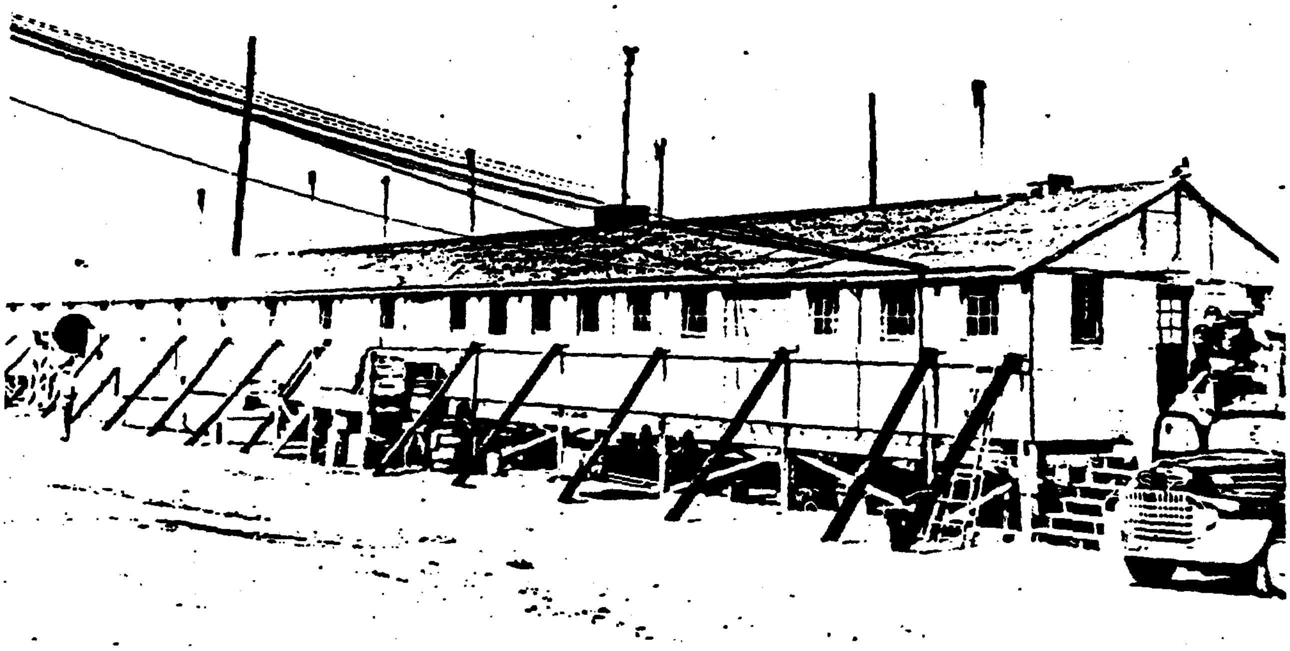

Figure 2.

Ranger control building--Frenchman Flat--Nevada Test Site. Note the shoring found necessary after the first shot to keep the building from collapsing.

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sample collector planes were based at Indian Springs. The administrative portion of the operation was kept in Las Vegas, some 70 miles away.

Thus, within a period of some eleven days (January 27 to February 6, 1951), five airdrops were made, allowing the data to be collected that led to the proper decisions for the Greenhouse devices. The time from conception of Operation Ranger to completion was approximately two months and the total operational cost to the AEC was some $3-1/2 million.

Thus, the combination of Operations Ranger and Greenhouse in late 1950 and early 1951 saw the beginnings of the thermonuclear burn weapon and the diagnostic techniques that went with it, the beginning of high-temperature x-ray measurements, the further development of reaction history techniques, and the proving of the operational concept of airdrops for experimental devices that was to be used so much in Nevada in later operations. These operations also tested the ingenuity of, and contributed strongly to the training of, those people who were to carry on in the later 1950s and establish the capability that carried us through the moratorium. Greenhouse also saw the first strong participation of UCRL in the weapons test programs. That Laboratory had, of course, contributed during the war years to all of the facets of the effort that later produced the atomic weapon.

The comparative ease and speed of the Ranger operation, combined with the growth of ideas for new and smaller devices, and the need for such devices as expressed by the military, led to the suggestion that a permanent proving ground for small-yield devices be established in Nevada. The experience of Ranger, in which the alpha blockhouse had been practically buried by fluffed-up dirt around the target area, led to the design of a more permanent installation in Yucca Flat. The firing site was moved from Frenchman Flat to Yucca Flat to get further away from the Las Vegas-Tonopah highway and from the new service town, Mercury. Time for planning also allowed the introduction of many more measurements, mostly output or effects measurements. Thus, the Buster-Jangle operation of the fall of 1951 in Nevada saw the continued development of the methods of conducting an airdrop operation against a fixed ground target. Fireball measurements became more sophisticated, especially with the introduction in 1952 of "Rapatronic" cameras, which took microsecond exposure pictures at predetermined times after the initial explosion. Radiochemical sampling and analysis methods were further improved. The growing Department of Defense need for effects data led to the Jangle surface and underground detonations for effects measurements.

The establishment of the Nevada Proving Ground in 1951 thus allowed a change in testing philosophy. Small devices (up to approximately 60 kt) would be fired in Nevada with the resultant saving in effort and money over the comparatively massive Pacific operations, and the Pacific would be used for tests of those devices that could not be safely fired in Nevada. There were, of course, exceptions to the latter part of this philosophy. A number of small shots were fired in the Pacific in later operations, because the sponsoring Laboratory did not feel it wise to wait for the next Nevada operation. With certain exceptions, a pattern grew of an operation once a year (1952-1958) alternating between the Pacific and Nevada. [redacted]

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The pleasant state of affairs in which the United States had "the bomb" to itself had ended in 1949, when the Russians tested their first device. While the concept of a thermonuclear device had been extant almost from the beginning of the Manhattan District project, work tended to concentrate on the fission device. The critical decision to accelerate the development of the thermonuclear weapon had been made by President Truman at the end of January 1950 under various political pressures including the strong desire of some to take another quantum step past the Soviets. [redacted]

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The first full-scale thermonuclear shot was of the Mike device of Operation Ivy on the island of Elugelab on the first day of November 1952 (Eniwetok time). Since the yield was expected to be large, new operational techniques were introduced. Most importantly, the firing control was aboard ship and the entire atoll was evacuated for the detonation. A month or so before the detonation, Edward Teller estimated that there was a remote chance that the yield might be as much as 100 megatons. The recognition of the possible effects of such a large yield led to the early calculations on the possible production of tsunamis (long wavelength ocean waves) and the need to take precautions in case they were produced. (Measurable tsunami waves were observed in Hawaii for most megaton-range Eniwetok Proving Ground shots, and this effect entered seriously into the safety considerations for the 1962 Pacific operation.) The expected yield of Mike also led to the requirement for higher-altitude wind predictions and soundings than previously needed, and the accompanying fallout predictions. (Mike was fired on the planned date, October 31 (CONUS time), which turned out to be the only day for about a month on each side that had adequate firing winds.)

The need to observe the phenomena of thermonuclear device functioning led to a new generation of experiment design on Ivy (which was expanded in Castle). In addition to observing the functioning of the primary in the normal manner, massive experiments were performed to observe [redacted] The techniques for observing the outputs (gamma rays, neutrons, light, etc.) were also refined. It is interesting to note that after the shot, the yield was uncertain for some time. The fireball measurements were suspect (in some circles) since this was the first large device fired on the ground's surface. The radiochemical results indicated a very low yield (1-2 Mt) initially because of the large natural background of uranium in the crater material. The first moderately correct number (10 Mt) came from the observation of the late-time gamma-ray intensity.

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Mike also added to the postshot experience of the task force personnel. The water wave and blast effects were noted and furnished a better basis for preshot planning in later operations. One omission was, however, to have a serious effect later. The fallout from Mike apparently went into a region that was not well instrumented and, hence, no good fallout observations were made at appreciable distances. AEC Headquarters, and in particular Merrill Eisenbud of the Health and Safety Office of the Biology and Medicine Division, had set up their own fallout-monitoring project to try to locate the fallout throughout the Pacific at various distances from the explosion. Although Eisenbud's organization flew extensive missions for three days after the event with various types of equipment, he reported their efforts to be largely unsuccessful in that they never found the main body of the fallout. "Whether it all shot into the stratosphere or not is one of the mysteries of the nuclear age."*

Ivy (1952) also saw the detonation of King (540 kt), [redacted] It was fired as an

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*Earl H. Voss, *Nuclear Ambush*, Henry Regnery Co., 1963, pages 33 and 34.

airdrop over Runit and added little to the testing capability except to show that such large yields could be safely airdropped. Curiously, history points out that the Russians got a propaganda advantage with the first airdrop of a thermonuclear device in 1955, but it is interesting that the U.S. had the first airdrop of a very high-yield (megaton range) device with the King test in 1952.

The next operation after Buster-Jangle, Tumbler-Snapper, in the spring of 1952 in Nevada, saw the rapid growth of Nevada testing techniques that were then to last, with only one major exception (airdrops), through the period before the moratorium. The experience of Ranger and Buster had quickly shown that while there was a strong advantage operationally and economically in airdrops, there were also a couple of serious disadvantages. The device to be tested had to be constructed to withstand the accelerations experienced during an airdrop and, hence, had to be much closer to the final stockpile construction than would be necessary to simply carry out an experiment. Furthermore, because of the uncertain position of burst owing to the inherent inaccuracies in bombing, it was not feasible to do detailed experiments on the operation of the device and specifically not feasible to do close-in measurements.* Thus, for Tumbler-Snapper, some half of the shots were placed on towers which then allowed detailed measurements of radiation flow, case operation, etc. The tower shots were operationally more difficult to fire because of the increased portion of the radioactivity to be expected in close-in fallout. The tower shots allowed more precise planning and positioning of the instruments and equipments now being fielded by AFSWP,** as a result of the increased interest in effects on the part of the Department of Defense. Thus tanks, jeeps, and pigs, as a function of distance, became a common sight in Nevada. Similarly, as a result of the recognition that we no longer had the sole offensive nuclear force in the world, the question of civil defense was taken up seriously for several years and these experiments in Nevada allowed the responsible organization (Federal Civil Defense Agency) to gain appreciable experience in understanding the effects of nuclear detonations on housing and buildings.

The loss of data due to the inherent inaccuracy in airdrops led to considerations of still other methods of testing in Nevada that would have some of the aspects of cheapness that the airdrops had. Also, there should be the advantage of a comparatively small amount of local fallout that would not require the great field efforts inherent in tower shots. Thus, Sandia, in conjunction with the two weapons design laboratories, developed the capability of lifting the experimental devices by tethered balloon. This method of emplacement replaced the airdrop system beginning with the Plumbbob operation in 1957 and continuing through Hardtack Phase II in 1958. The balloon system was eminently satisfactory in that it kept the fallout to a minimum, allowed some close-in alpha measurements, was fairly inexpensive, and allowed effects experiments from devices going off at almost militarily optimum altitudes. That method also allowed the gear around the device to be placed in a comparatively haphazard fashion, hence reducing the effort required of the bomb packaging people.

The large crater produced by Mike shot in Operation Ivy made it obvious that the Marshall Islands could not support a long series of high-yield shots fired in that manner, with the inevitable destruction of Marshallese homelands. Thus, the Castle operation in the Marshall Islands in 1954 saw the beginning of a testing technique that was to last through the rest of the operations at the Eniwetok Proving Ground.

*Except for some telemetry.

**Armed Forces Special Weapons Project.

In that operation many shots were fired on large barges which allowed moderate instrumentation, some careful pointing, and hard wire timing and firing, but which would not produce large craters to do away with the land area, and in some cases would allow reuse, either in that operation or another, of the recording facilities. The barges could be positioned with sufficient accuracy so that collimated systems on shore, in conjunction with appropriate shielding on the barge, allowed observation of specific portions of the device.

The shots of Operation Castle were designed to produce an emergency capability for the United States since the Russians had just tested their first thermonuclear device and, by now, clearly had fission devices in stockpile. The shots were almost all large-yield thermonuclear devices that, in general, produced yields somewhat different than those expected. The Bravo shot, specifically, went to 15 or 16 megatons, as opposed to the predicted 6 megatons and produced fallout that extended to Rongelap and Utirik, where there were native populations, and to Rongerik, where a Task Force weather station was sited. It was probably this large population exposure to radiation, in combination with other things, that led to the beginning of the real pressure to stop atmospheric testing. Castle also saw the reopening of Bikini as a test site. This came about in order to increase the number of acceptable firing days from the point of view of weather and also to give sufficient land surface for further shots to be fired in fixed positions on the land. Again, UCRL contributed heavily to the diagnostics performed on Castle, in addition to firing the first Pacific shot of their own.

At Castle, a hard wire timing and firing system was reinstituted. Hard wire distribution systems were placed around the major portions of both atolls and firing was done from the shore-based control stations. In the case of Eniwetok, the control station was on Parry, and it was on Enyu for Bikini. After the Bravo shot in Operation Castle, it was necessary to go back to a ship-based operation at Bikini because the atoll was too radioactive for safe occupation. However, the shots were still fired from the timing station on Enyu.

Operation Redwing in 1956 and Hardtack Phase I in 1958 at the Eniwetok Proving Ground were then conducted in essentially the same fashion as Castle, as far as development shots were concerned. The diagnostic techniques were refined and changed during that period, but the general philosophy of the method of testing and placement remained the same with minor variations. During Redwing and Hardtack, the Atomic Energy Commission, specifically Libby, insisted that we put an appreciable amount of silica sand in the barges in hopes that such sand would increase the concentration of local fallout and, hence, remove some of the hazards from long-range fallout. He also hoped that more of the strontium would appear as the insoluble silicate, hence reducing the problem of ingestion of long-range fallout. Evidence indicates that this had no particular effect. In a similar vein, during Operation Hardtack, Oak was moved from comparatively deep mooring at Bikini Atoll to a position on the reef at Eniwetok Atoll in order to increase even further the proportion of solids in the radioactive cloud, and to change the strontium compounds formed.

Over this period of time, some shots were fired by other methods, largely for Department of Defense effects purposes. The Navy continued its investigation of the effects on ships of underwater detonations, conducting in 1955 the Wigwam shot at a point in deep water 600 miles off the California coast and continuing variations of that during Hardtack Phase I, with shots in the lagoon or just out of the lagoon at the Eniwetok Proving Ground. The early interest in the effects of high-altitude shots is shown by the HA shot in Teapot in June of 1955, a 3-kt airdrop detonated at 36,620 feet; and the Yucca shot in Hardtack Phase I, a [redacted] balloon-lofted detonation at 86,000 feet.

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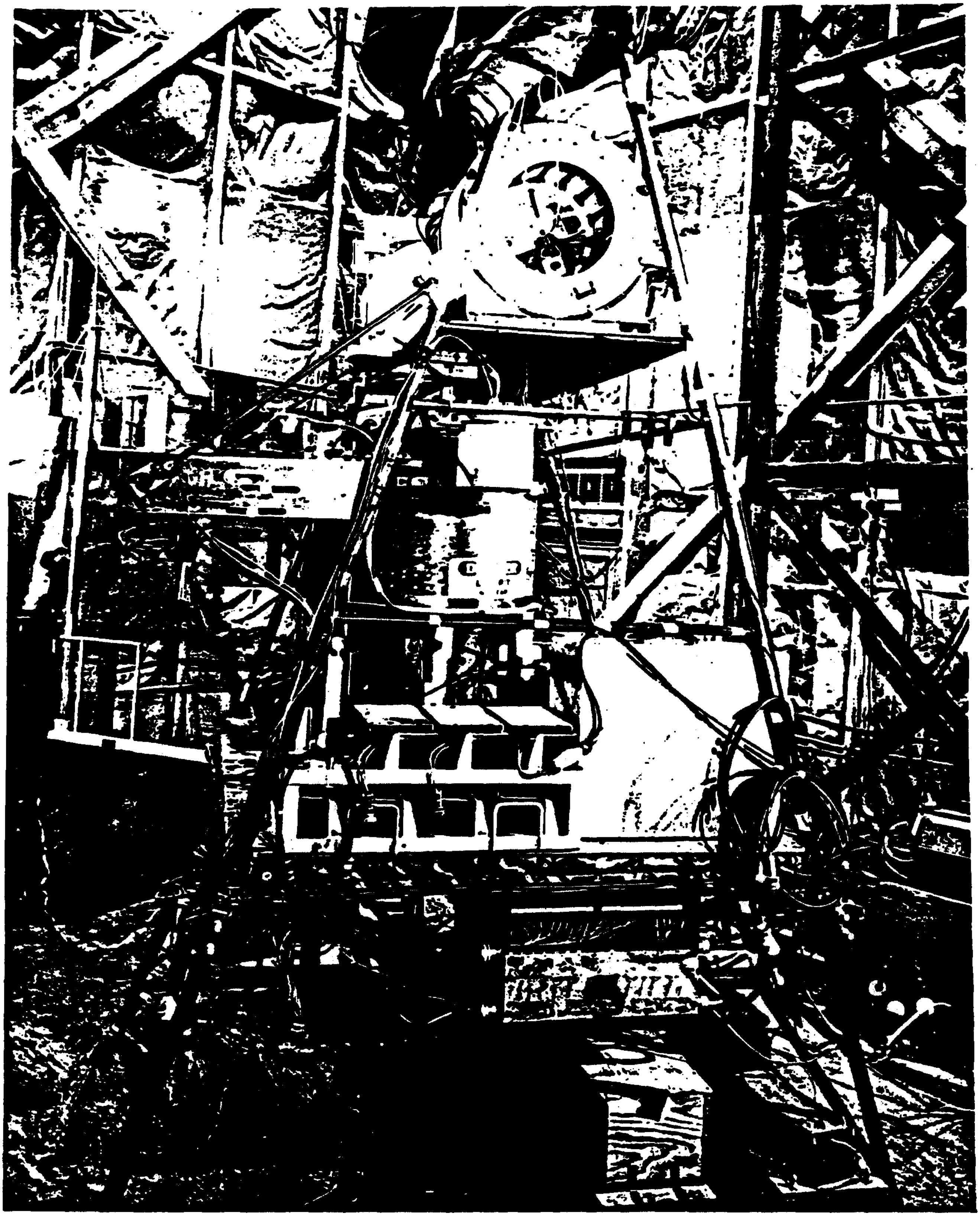

Figure 3.
Typical "balloon cab." The device is under the sign "Live Pit."

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More significant, however, from a testing viewpoint, were the Teak and Orange shots of Operation Hardtack in 1958 and the Argus shots. Teak and Orange were conceptually initiated by the AFSWP (and the Air Force) during the period in which the U.S. was considering the Nike-Zeus system as the early ABM system. It was realized that essentially nothing was known about the effects of large-yield, high-altitude detonations. These two shots were originally planned as roughly [redacted] detonations to take place at approximately 250,000 feet and 125,000 feet (76 and 38 km) and to be fired above Bikini. However, safety studies conducted during the early part of the Hardtack Phase I operation showed that there was appreciable hazard of eyeburn to the Marshallese natives if those detonations took place there. Therefore, late in the Hardtack Phase I operation, the shots were moved to Johnston Island. The launch pad for the Redstone missile was put in and they were detonated at high altitude over Johnston Island two months after the decision to move. This was the beginning of the use of that atoll as a launch site for high-altitude detonations.

Early in 1958, Nick Christofilos of LRL realized that there was a possibility of electrons from high-altitude detonations being trapped in the earth's magnetic field and oscillating back and forth along the field lines, thus artifically producing a shell or shells of high electron densities over much of the earth. Such a phenomenon might have useful military applications. In order to test this theory quickly before the test moratorium went into effect, the Department of Defense arranged the very secret Argus series, which was conducted by a Naval task force (Task Force 88) in August and September of 1958 in the South Atlantic. This resulted in three 1.7-kt detonations at altitudes ranging from about 100 to just over 400 nautical miles. The

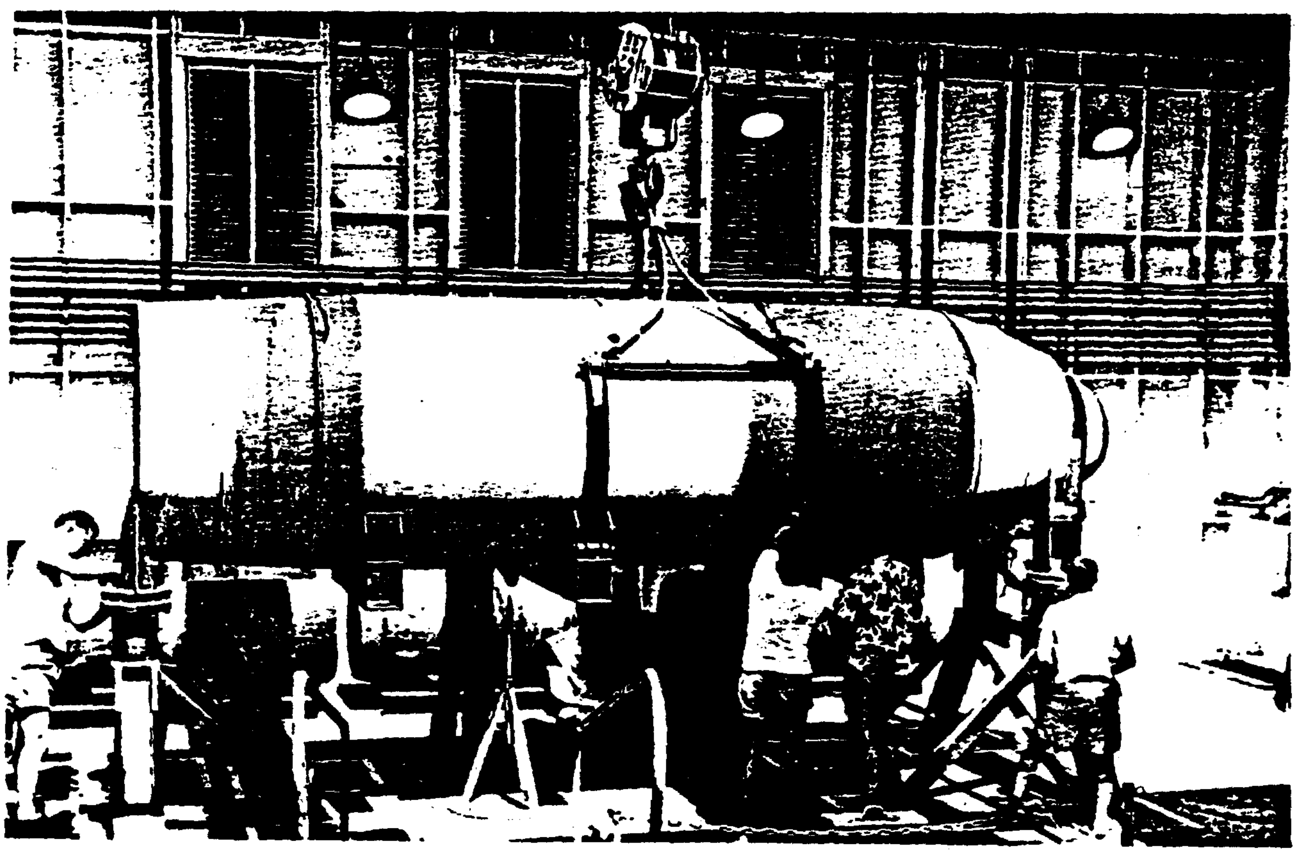

Figure 4.
Operation Castle device being lowered into place on a barge.

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devices were carried aloft from a surface ship by a Lockheed X-17a three-stage rocket, making these the only ship-launched, high-altitude, rocketborne nuclear detonations that the U.S. has ever performed. In addition to observations and measurements made from land, ships, and aircraft, instruments carried by sounding rockets and one of the first U.S. earth satellites (Explorer IV) provided useful Argus data.

During the period of 1956-1958, the concept of doing nuclear weapon testing underground received more and more attention, especially by Edward Teller, as a possible solution to some of the test ban debates. Firing underground would presumably allow continued weapons testing without the concomitant fallout problem that was, in some circles, regarded as one of the major difficulties with nuclear weapons testing, or conversely, one of the major reasons for stopping such testing. Thus, in 1957, the Livermore Laboratory conducted the 1.8-kt Rainier shot in a tunnel 900 feet below Rainier Mesa at the Nevada Test Site to investigate the conditions of containment of underground shots. Containment of that detonation was a success, with no tunnel venting. Livermore also conducted, during Hardtack Phase II, four other tunnel shots with moderate success. These shots were fired in tunnels mined into the Rainier Mesa. Over the same period of time, Los Alamos had conducted a couple of very small-yield safety shots in vertical drill holes in Yucca Flat. [redacted] Those two shots were stemmed by a concrete plug (precast) just above the device on the bottom of the hole and another very small plug at the top of the hole. From their early underground detonations, Livermore apparently learned that the tunnel was convenient, instrumentation could be placed at various angles around the device, the device could be worked on in place, and the stemming did not seem to be awfully difficult. Los Alamos seems to have learned that drilling holes was cheaper than mining, but that stemming could be a serious problem.

Yet another type of nuclear test operation requiring deployment to the field was initiated and continued through these years. Such tests came to be known as one-point or safety tests. The first of these was done at the NTS at the beginning of November 1955. Three tests at this time and a fourth in January of 1956 were all given the title "56 Project--NTS." A test organization was set up with the AEC Test Manager at NTS having overall responsibility and the tests being carried out by a LASL team headed by their own test director, since these were LASL devices. The different devices containing their normal high explosive [redacted] were detonated at a single point by a standard detonator properly situated [redacted] Further, to ensure a "worst case" situation, the nuclear fuel quantity was at least as great as the maximum that would occur in production. A neutron source provided neutrons sufficient to assure initiation of a reaction [redacted] The objective was to demonstrate that each of the devices was "safe" for this mode of detonation, leading to a nuclear reaction no greater than the equivalent of a few pounds of high explosive. These tests did demonstrate the "one-point safety" of three different LASL devices.

Now that the one-point safety tests became a normal part of the various test series, the next question of weapons safety, that of plutonium contamination, came to the fore. The health hazard from inhalation of plutonium (an alpha emitter) is quite

*1 shake = 10^{-8} seconds.

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serious and the possibility of exposure to this from the various stages of handling of weapons containing plutonium had to be thoroughly evaluated. This resulted in giving Sandia Corporation the responsibility for the "TG 57 program" to perform tests and measurements to further understand the plutonium scattering and contamination characteristics. Thus, a plutonium weapon was single-point detonated in late April 1957 and, through numerous means, Sandia and their support contractors gained further understanding of the patterns of plutonium scattering, fixation, decontamination, and other data.

Several other single-point safety tests were carried out by LASL and Livermore, respectively, in "Project 58--NTS" and "Project 58A--NTS" in late 1957 and early 1958. Other safety tests were incorporated in larger series of various weapons tests.

During the last part of Hardtack Phase II in Nevada, when the test organization was frantic for emplacement positions and was firing several devices a day, a few devices were being detonated in so-called "Gravel Gerties." These were simply small buildings on the surface of the ground with a great amount of dirt piled over them as emplacement positions for very small-yield safety shots, the concept being that the dirt would scavenge the radioactive material and bring it down very close to the zero point and thus prevent off-site fallout. This apparently did help to some extent.

Thus, by late 1958, at the beginning of the moratorium, the test organization had learned to test bombs using a number of different placement methods: airdrops, balloons, towers, barges, and surface shots; and had developed what seemed to it the best methods of so doing, consistent with the characteristics of the test sites it was using. It also had some experience with rocket launching of devices. [redacted] The test organization had developed some capability for underground testing, but had not pursued that technique far enough to be confident of either the economics or the containment features, or far enough to be confident that all of the necessary diagnostics could be done in a satisfactory fashion. It had conducted a fair number of underwater shots for Navy purposes and was moderately confident of those methods. It could not claim to really understand fallout, but had models to predict the fallout pattern sufficient for operational purposes and knew what kind of weather information was needed. However, it is again to be pointed out that practically all of this experience had to do with surface shots and tower shots. The cloud formation from a venting underground shot is obviously a somewhat different beast. Most importantly, the test organization had by then a great deal of experience in the safety precautions to be taken when nuclear detonations are to take place and had a cadre of people experienced in this field.

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Table II lists the operations of 1945 through 1958. The "operational period" normally began about a month before the first planned shot date and ended perhaps a week after the last detonation.

AEC Device Diagnostic Standard Measurements

As time went on in the period of 1946 to 1958, the device designs produced by Los Alamos and Livermore grew in complexity, sizes decreased so the time constants changed and, hence, the requirement for more and more detailed diagnostics grew. We will, in this section, comment mainly on the type of basic diagnostics that had to be performed in the early period after the moratorium to make it useful to fire the shots at all. We will also comment to a certain extent on the more complex measurements that had been developed during the period of 1946 to 1958, simply to illustrate

TABLE II
NUCLEAR WEAPON TEST OPERATIONS[a]

Operation	Dates	Location
Trinity	7/16/45	Alamogordo, New Mexico
Crossroads	6/30-7/7/46	Bikini Atoll, Marshall Islands
Sandstone	4/14-5/14/48	Eniwetok Atoll, Marshall Islands
Ranger	1/27-2/6/51	Nevada Test Site
Greenhouse	4/7-5/24/51	Eniwetok Atoll, Marshall Islands
Buster-Jangle	10/22-11/29/51	Nevada Test Site
Tumbler-Snapper	4/1-6/5/52	Nevada Test Site
Ivy	10/31-11/15/52	Eniwetok Atoll, Marshall Islands
Upshot-Knothole	3/17-6/4/53	Nevada Test Site
Castle	2/28-5/13/54	Eniwetok Proving Ground
Teapot	2/18-5/15/55	Nevada Test Site
Wigwam	4/14/55	29^{o} N, 126^{o}W
Project 56	11/1/55-1/18/56	Nevada Test Site
Redwing	5/4-7/21/56	Eniwetok Proving Ground
Project 57	4/27/57	Nevada Test Site
Plumbbob	5/28-10/7/57	Nevada Test Site
Project 58--NTS	12/6-9/57	Nevada Test Site
Project 58A--NTS	2/22-3/14/58	Nevada Test Site
Hardtack Phase I	4/28-8/12/58	Eniwetok Proving Ground and Johnston Island
Argus	8/27-9/6/58	South Atlantic
Hardtack Phase II	9/19-10/31/58	Nevada Test Site

[a]The Hiroshima and Nagasaki detonations of World War II were August 5 and August 9, 1945, respectively (Greenwich Civil Time).

the kinds of things we could not immediately do in 1961 as a result of our very quick return to testing with very little preparation, and perhaps more pertinently, very little experience with the new techniques of testing required.

For the normal fission device with no boosting and no secondary, in the very early years of testing, two quantities were of prime importance. The first of these was the energy release, or "yield," of the device, which was directly related to the efficiency of burn of the fissionable material. In the early years, specifically around the time of Trinity, Crossroads, and Sandstone, the experimenters let their imagination run riot to imagine experiments that would give them a handle on the yield. At Trinity, a number of esoteric measurements were made. Fermi estimated the yield of the bomb by simply observing the motion of some scraps of paper he dropped from his hand as the blast wave went by. He had calculated the duration of the positive phase with respect to yield, and by simply observing the time it took for the wind to reverse and knowing his distance from the bomb, he could make an estimate of the yield. Measurements of the various outputs were made at Trinity in order to get a handle on the same subject. The neutron flux in various energy regions was measured. The gamma ray output, the integral of the total light, and the light curve were all measured, but without previous experience they could not be particularly trusted as a measurement. They did all establish the range of yield. That is, the experiments could probably determine without much question, with the possible exception of the gamma curve, which was misunderstood, that the yield was somewhere between 10 and 30 kilotons, but they could not pinpoint it.

Radiochemistry/Sampling

The primary method of measuring yield used at Trinity and depended upon more than any other method clear through the 1958 period, is that normally called "radiochemistry." In principle, the concept was very straightforward. After detonation, a portion of the condensed, particulate debris from the detonation was collected and taken to the laboratory. In the laboratory, chemistry was used to separate out of the debris not only the active material that had been used, such as plutonium or uranium, but also certain representative radioactive fission fragments. An observation of the amounts of both of those materials in one sample then allowed a determination of what proportion of the active material had burned. Knowing that and the amount of active material in the bomb, it was possible to then calculate the energy release based on laboratory measurements of the energy developed by one fission. In practice, the system was not quite as simple as this. In the uranium-type devices, there could be some contamination from the uranium in the ground. There was not always uniform mixing in the cloud. There is some small uncertainty as to the energy release per fission, since it is dependent not only on the isotope, but on the energy of the neutron producing the fission. The advent of the thermonuclear bomb increased the complexity of the knowledge necessary to properly interpret the results. The fate of the neutrons produced in the thermonuclear reaction can have a significant effect on the yield.

Samples were collected at Trinity using ground-based air samplers. Samples were also collected of the fallout, but they were not particularly useful. In normal atmospheric testing, the general procedure was to wait for some appreciable length of time (1/2 to 2 hours) until the cloud had mixed (theoretically uniformly) due to its heat-generated turbulent action so that upon sampling, any sample collected would presumably be representative of the whole bomb. The assumption of uniform mixing was not taken on faith. The entire history of the period 1946 through 1958 is one of trying to establish that assumption or, where it seemed to be questionable, to find methods of handling the situation. For example, for most detonations, it was common to collect samples from several portions of the cloud chosen ahead of time by the experienced Laboratory representative in charge of sample collection. The samples were then treated separately to observe any fractionation that might be present, but were lumped together to conclude the results of the detonation.

It was also necessary to let the cloud diffuse and "cool" for a similar period of time (1/2 to 2 hours) in order that the activity would reduce to levels that made it acceptable to send manned aircraft into the cloud. Even at that time, it was quite possible to get into "hot" regions of the cloud or to overstay the appropriate time to such an extent that unacceptable crew doses would be obtained and, therefore, it was a necessary function of the scientific controller to watch penetrations with great care to make sure that no overexposures were experienced. Since on most aircraft the major dose to the pilot could come from the sample collected either in the sampling collectors or in the engines, it was also necessary to prejudge very carefully how much would be collected in order that the aircraft could return to base before the pilot was overdosed. Obviously, appropriate aircraft washdown facilities had to be developed. Several times during this period the sampling tanks had to be redesigned as new aircraft were devoted to this function. Specifically, it was necessary to design tanks to fit the operational speed of the aircraft and still allow the air to pass through the collectors at low enough velocity so the filter papers could handle it; otherwise, mechanical tearing could result. The filters were designed to allow fission particles of all various sizes to be collected with equal efficiency (isohenticity). The design of such collectors was a very large job performed by external contractors with technical guidance from the Laboratory.

Because of the large operational cost of sampling with aircraft, the large dose to the pilots, and the usual shortage of aircraft, several attempts were made during this period to develop quicker or less costly sampling systems. B-17s were converted into drones and, guided from a mother ship, were used in some of the early overseas operations (Crossroads and Sandstone). However, they were very hard to control and a number were lost. They could not reach required altitudes and were very expensive to operate. During Greenhouse a fairly expensive attempt was made to obtain so-called "grab samples." Very large steel devices were constructed, which were placed close to the base of the tower. These devices were designed so that after the first shock wave went by and the device was enclosed in the fireball, a large valve would close, trapping an appropriate amount of the active material inside. Presumably then, at a later time, these "bottles," some of which resembled gun barrels, could be recovered and the sample treated. Unfortunately, the valves in general did not work, no samples were collected, and the method failed. In at least one case the bomb yield was larger than the "bottles" were designed for, and they were destroyed. At Trinity, the soil around the tower, which contained silica, had melted inside the fireball and plated out as a glass on the ground. This glassy material contained enough of the radioactive debris that it was useful as a sample of the bomb. Various attempts were made at Sandstone and Greenhouse to reproduce this by spreading great numbers of broken beer bottles and other silica-containing material around the towers. This, in general, also failed because the materials were blown away. On at least one occasion, it failed because the beer bottles turned out to be plastic. As part of this same trend of thought, a radio-controlled vehicle was obtained for Sandstone to enter the crater early and recover appreciable amounts of the material around the tower. Likewise, this method failed because the recovery system did not work very well and the material at the base of the tower did not contain the required fission debris. A variation of this system had been used at Trinity, in which a manned tank with a remotely controlled bucket on the front of it had been used. The tank got stuck in the crater and caused quite a furor.*

An attempt was made to sample the radioactive cloud at Trinity using filters on B-29s. However, the aircraft did not operate, so no samples were collected.

At Greenhouse (1951) LASL again tried to collect airborne debris by firing 5-inch HVAR rockets with sampling heads developed by China Lake from one island across the shot island to a third island. The rockets were fired a few seconds after nuclear device detonation, and were in general deflected by the shock wave and lost. A few were recovered but the samples were not adequate.

Livermore tackled the rocket sampling problem once more in the late 1950s with the use of small rockets outfitted with a sampler head which was designed to intercept the cloud, take a sample, and then close and parachute to the earth. The collecting heads were built to float and, in some cases, built with small beacons and sea dye so that they could be found in water. This system was tried over several operations in the Pacific and was a forerunner of the "Cleansweep" system tried at Dominic. Since the attempt was to get early samples, these rockets were fired soon after the detonation (five or six minutes), and in some of the early attempts encountered sufficient turbulence in the cloud that they were thrown off course, broken up, or otherwise not recovered. By the time of the moratorium, the rocket sampling system was showing considerable promise, but had not yet been developed into a dependable, operational system.

*The tank was driven by Sgt. Bill Smith and carried Herb Anderson and Enrico Fermi.

The need to obtain samples from the one-point safety tests of Project 56 led to the design of collectors intended to pick up the large particles of active material that might be expected from very low or zero-yield devices. These collectors consisted of sand-filled wooden boxes, about 4 feet square and 10 feet long, with the long axis radial to the bomb. In principle, the heavy particles would enter the sand and stop. Later "sand sifting" would then reveal the sample. The technique worked when the yield was not appreciably larger than that predicted.

As development efforts changed from fission bombs to large thermonuclear bombs, the pressure grew to obtain a higher-altitude sampling capability. In the case of surface and barge shots, this was necessary simply in order to make sure that representative samples were being obtained. In the case of airdrops, the bottom of the cloud might well rise up to the tropopause or higher and, hence, reach an altitude that could not be reached by some of the earlier sampler aircraft. A list of aircraft used and aircraft characteristics versus time is given in Table III.

TABLE III
AIRCRAFT USED FOR THE COLLECTION OF RADIOCHEMICAL BOMB DEBRIS SAMPLES FOR AEC LABORATORY USE, PREMORATORIUM

Operation	Date	Aircraft
Trinity	1945	None
Crossroads	1946	B-17 Drone, Navy F6F Drone
Sandstone	1948	B-17 Drones
Ranger	1951	T-33
Greenhouse	1951	B-17 Drones, F-80 Drones, B-29
Buster-Jangle	1951	T-33, B-29
Tumbler-Snapper	1952	F-84, B-29, T-33
Ivy	1952	F-84G, B-29, B-36
Upshot-Knothole	1953	F-84G, B-29, B-36
Castle	1954	B-36, F-84G
Teapot	1955	F-84G, B-57A (B-50D Controller)
Redwing	1956	B-57B, F-84G
Plumbbob	1957	B-57B, F-84G, T-33?
58-NTS	1957-58	B-57B
Hardtack	1958	B-57B, B-57D
Hardtack II	1958	B-57B

By the beginning of the moratorium, there was available a quite satisfactory sampling system for normal detonations. The system consisted of the B-57 aircraft in several configurations, with the appropriately designed sampling apparatus. Over this period of time, an Air Force organization, first designated AFOAT-1 and then AFTAC*, had been developing aircraft sampling systems for remote detection of foreign tests. These were first used on the B-29s and later in other aircraft. They eventually developed equipment and instrumentation for use on WC-135As at low altitudes and the U-2 at high altitudes. Their collection systems, however, did not, in

*AFTAC--Air Force Technical Applications Center.

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general take sufficiently large samples to be adequate for the detailed analysis required by the weapons design laboratories, or were limited to long-range bomb cloud sampling by design.

The advent of the thermonuclear bomb, with its large energy release from nonfission fuels, led to the problem of determining the thermonuclear burn. One possible solution to this problem was to collect and analyze the gaseous products of fusion. In the mid-1950s, such gas collection systems were designed by Livermore and AFTAC for use on the sampling aircraft. The samples collected were not of particular value to bomb diagnostics during that period of time. However, these efforts led to an eventual capability for collecting and analyzing such samples, postmoratorium, and have been especially valuable in establishing techniques which are used today with respect to foreign tests.

Over this same period in time, the capability of the radiochemical technique for weapon diagnostics grew greatly. Initially, the technique was thought of only as the fission yield measurement derived from the ratio of fission fragment production to active material in the sample. This simplicity became disturbed very quickly with the introduction of composite devices that had both uranium-235 and plutonium as fuels, because of the variable partition of fission between the uranium and plutonium materials. However, this introduced little more than a complication in the arithmetic with which the data were treated, although sensitive fission particle analysis helped. It was also recognized early-on that the energy release per fission was dependent upon the energy of the neutron causing the fission. Correct treatment of the incident neutron spectrum was required, and was initially based upon calculations of the neutron energy distribution through the device. [redacted] This measurement then prompted a more detailed correction for the energy spectrum in calculating the yield. Cross calibration between the Los Alamos Scientific Laboratory, AFTAC, the British, and later the Lawrence Livermore Laboratory assured the validity of the primary radiochemical processes and philosophies. [redacted]

Since most of the devices contain ordinary uranium, or even depleted uranium, some fission takes place in that material due to the high-energy end of the neutron spectrum and, hence, these materials contribute to the energy release of the device. Correction for this phenomenon was initially made using the theoretical calculations of the neutron distribution. Appreciable difficulty was, however, experienced in the early Eniwetok shots because the natural uranium in the soil mixed with the bomb uranium in the cloud, making it difficult to determine the amount of bomb uranium in the sample. In some of the shots, this was overcome by simply putting barrels of a uranium compound close to the bomb before it was fired. Sufficient uranium of known quantity was therefore in the cloud to mask the background of the natural uranium. Later on, some of the tracers mentioned earlier were placed in the device uranium to obviate this problem. [redacted]

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[redacted] Lithium is present in fairly large amounts in the soil and ocean water of the Eniwetok Proving Ground. Deuterium is present in large amounts in the ocean water. Thus, while it was possible in the late 1950s to obtain samples of the gas from the radioactive cloud, it was difficult to determine what portion of the bomb was present in that sample and, hence, difficult to determine what amount of burn products were in the sample. As mentioned earlier, the radiochemical results on Mike shot, the first large thermonuclear device, were very uncertain because of factors of this sort. Attempts were made to solve this problem by noting the burn of the fissionable materials [redacted] and from that observation calculating the neutron flux that material was subjected to and in turn calculating the thermonuclear burn that must have produced the flux. [redacted] However, up to the time of the moratorium, the problem had not been completely solved.

The very large neutron fluxes present in the secondaries of thermonuclear devices resulted in a number of the fission products formed being transformed again because of neutron capture. An understanding of this "burnback" phenomenon was necessary to deduce the correct yield from radiochemical data. This requirement led to the development of detectors of appreciably different cross sections for neutron capture to be placed in the active material. The results from these detectors, combined with intricate arithmetic, usually led to a correction for the burnback in the samples.

Obviously, during this period, the laboratory techniques for handling radiochemical material, for counting and data treatment, including automatic inputs to computers, etc., were continually improved.

In 1957 and 1958, the weapon design laboratories, especially Livermore, began to gain experience on the problems of doing radiochemical analysis of the debris from underground detonations. Several difficulties were apparent. To drill back to the detonation region and obtain a sample was not only expensive but required the development of techniques for drilling and handling the radioactive material without creating a hazard to the operating personnel. The phenomena of underground cavity growth, melting, and resolidification were not well understood. There was no assurance that the samples obtained would be representative of the whole bomb. In fact, it was perfectly clear that in some cases, they would not be representative. This, if properly treated, could be an advantage, but clearly there was a whole new phenomenology to learn. When a bomb is fired underground, a large fraction of the neutrons go out into the dirt around the hole and some of those reflected back in may cause further burn of the fissionable material. Methods of either accounting for this or preventing it, in general, were not developed by the time of the moratorium. The whole question of how to handle thermonuclear burn appeared even more difficult under these conditions, but no solution was attempted before the moratorium. A little about handling large dirt samples was learned as a result of the few underground tests.

Fireball Yield

At the time of Trinity, the general equations for the growth of the fireball as a function of time and of yield had been worked out for an explosion in a simple gas. However, the details of shock-wave expansion in air at very high pressures were uncertain theoretically, and some of the required gas constants of air were not known. Furthermore, any effects of nuclear and thermal radiation on the air before the shock wave hit it were not understood. Most particularly, the calculations did not include the effect of the medium, during early expansion, consisting of bomb fragments, tower fragments, old high explosive, etc., rather than of pure air. Thus, in the period of 1945 through 1949, it was thought feasible to scale the yield by observing the rate of growth of the fireball from one detonation to another, but it was not considered feasible to determine the absolute value of the yield from this measurement. In general, it was thought than any characteristic phenomenon in the expansion, for example, the time of breakaway,* would scale as the yield to the 1/3 power. A rough scaling law for fireball growth as a function of yield at this time, derived from the simultaneity solution of the equation of motion, was that the yield was a constant times d^5/t^2, where t is the time taken by the fireball to grow to a diameter d. The solution applies after the fireball has encompassed a mass of air large compared to the mass of the bomb. Thus, in determining the yield, an error in the diameter measurement resulted in five times that error in the yield, and an error in the time resulted in twice that error in the yield.

While photographs of the fireball as a function of time had been taken during Trinity and Crossroads Able, there were problems in later interpretation. The Trinity films were not stored in such a fashion as to make later quantitative measurements completely reliable. Trinity was essentially a ground burst (100 foot tower), but the energy loss to the ground was small. For Crossroads Able, it was difficult even to determine the distance from the camera to the detonation. However, these pictures did allow an initial determination of the constants in the fireball rate of growth equations by comparing the results of such arithmetic with the radiochemical yields determined on Trinity and Crossroads Able. Because of timing signal problems, only streak camera records came out of Crossroads Able.

In order to conduct the Sandstone operation in 1948, the Los Alamos Laboratory set up a temporary task group, under Darol Froman, which allowed some appreciable preplanning and as a result, fireball rate of growth pictures with moderate time resolution were taken during Sandstone, largely under the guidance of Lou Fussel, later of EG&G, and Berlyn Brixner.

However, in 1949, with the formation of a permanent test division at Los Alamos, there began a serious attack, both theoretical and experimental, on this problem. Fred Reines, who was in charge of the experimental portion of the test division work under Al Graves, felt very strongly that it should be possible to make fireball measurements into an absolute yield measurement. He, therefore, set up a section within the division with people such as Fran Porzel and Joe Mullaney to pursue the theoretical aspects of shockwave expansion under these conditions and of the characteristics of air. They, in turn, sought the aid of other experts, particularly Hirshfelder and McGee. At the same time, there was established a relationship with the newly formed company Edgerton, Germeshausen, and Grier (EG&G), which was to expand the capability of detailed fireball measurements, reduce the time uncertainty, etc., throughout the entire period from 1949 to 1958. Porzel worked long and hard on the "analytic solution" to fireball growth and shock formation. While his solution was in fact semiempirical,** it was useful, both in this field and also in the basic understanding of blast phenomenology. The characteristics of shock formation and propagation in air at high temperatures, and specifically the constants that go with that, were studied theoretically and calculated to a much higher accuracy using the

*The separation of the shock front from the fireball.

**The solution used Fuchs' IBM M problem as a guide.

growing computer facilities at Los Alamos. The effect of the mass of the bomb and tower or local surroundings was clearly noted in the detailed pictures of Greenhouse and later operations. A theory to account for this mass was eventually developed and proven out against the field data. The group in Los Alamos grew over this period so that by 1958 there were several competent people in this field. In the period from its formation to 1958, Livermore also contributed to this effort. However, in general, they took the position that the measurements were well in hand and that their efforts were better spent on other portions of the diagnostics problem.

The knowledge of fireball growth at very early time gained from the streak camera data taken by both the Naval Research Laboratory (NRL) and LASL, assisted appreciably in the understanding of early fireball growth and, hence, all fireball growth.

EG&G, in their partnership with LASL, over this period of time developed experimental techniques that resulted in excellent fireball pictures. They developed the "Rapatronic" camera, which allowed single-shot pictures at preestablished times during fireball growth. Cameras were triggered from the first Teller* light and, hence, could take pictures with something like microsecond accuracy; however, only a few pictures could be taken for any given fireball because each camera only took one exposure. Standard and controlled development techniques were established to assist in determining the edge of the fireball with adequate accuracy and consistency. A storage system was set up in order that the film could be kept under the proper humidity and temperature so that later measurements would be significant. New films were developed with the aid of the manufacturers to better handle the wide ranges of brightness experienced. A team of film readers had been developed and trained at EG&G so that there was consistency in the reading of a given film independent of who read it. The Polaroid process was applied to some cameras to allow early fireball yield determinations. Camera timing was improved so that the inaccuracies in yield, because of timing inaccuracies, could be reduced.

By 1958, the field expertise and the theoretical understanding had reached such a point that the fireball measurement was regarded as "the yield measurement" for thermonuclear bombs and there was a running debate as to whether it matched radiochemistry for normal fission bombs.

Thus, in the latter part of 1958, there was a highly trained crew in EG&G for taking field fireball measurements with a great amount of gear such as cameras, timing systems, etc., for that purpose, and in the Laboratories, a good understanding of fireball phenomenology and the relationship between fireball growth and the energy release of the device. Obviously, this well-established technique for determining yield, and specifically the prime technique for determining the yield of thermonuclear devices, could not be used underground.

The Reaction History

Alpha

Early-on, the term reaction history usually referred simply to the measurement of the exponent in the equation $I = I_0 e^{\alpha t}$ where I_0 could be taken as the flux of neutrons at a given point in the bomb or gamma rays external to the bomb. The equation could also be written as an integral such as $N = N_0 \int^t e^{\alpha t}\, dt$, where N could

*See later section on Alpha.

be the neutron flux in the bomb, N_0 the initial flux which might be established by the natural background or by an artificial source, and t the time from the beginning of the nuclear reaction. However, in the period of 1945 to 1958, other phenomena were introduced and the term reaction history was used to cover the measurement of all of these quantities. [redacted]

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The point of the early reaction history or, as commonly called, alpha measurement, was to determine the rate of increase of the population of neutrons in the device, both resulting from fission and causing further fission, and hence check the "criticality" calculations. Since the neutrons were not immediately available for observation (they were inside the device), and furthermore since they do not travel at the speed of light, by the time they get outside the device, there is some time smearing in the detection of the neutron flux, which depends upon the neutron spectrum. Obviously, the time smearing did not matter as long as the spectrum of neutrons in the device remained constant and alpha was sensibly constant. However, we were not sure of such constancy. There were other difficulties mitigating against the use of neutrons and some which even caused problems in observing gamma rays. [redacted] the cable carrying an electrical signal from a detector could short out very quickly after the detector received the gamma ray signal. Since the neutrons traveled at a speed appreciably less than the velocity of light, the cable taking an electrical signal from a neutron detector could be shorted from the high-intensity gamma flux before the neutrons could reach the detector. There were several more difficulties recognized before Trinity by the people involved with this measurement, mainly Bruno Rossi and Bob Wilson. The signal was so fast that the recording equipment of the time would possibly not write. The problem of oscilloscope presentation was difficult. When presented in a normal fashion with a linear sweep and the signal vertical, the signal would sweep off the scope before an appropriate measurement could be made. A beam intensifier had to be used to increase the writing speed, but that implied turning on the intensifier at just the right time, and the vacuum tube circuits in those days were not reliable enough to guarantee the time delay. A self-triggering system had to be developed since the observation would only take a microsecond or less on any given oscilloscope, but this was occurring 100 microseconds or more after detonation of the high explosive. Since the time between high-explosive detonation and criticality was uncertain by more than a microsecond, the scopes could not be triggered by the high-explosive signal. The yield of the bomb was expected to be such that a recording station had to be placed an appreciable distance from the bomb, some 1,000 to 2,000 yards, and, hence, the attenuation of the transmission cable then available would be extremely high for signals of the expected rise time. Because of the philosophical difference between a pure exponential and a sine wave, and because of the lack of clearly appropriate exponential signal generators it was not even clear the cable attenuation could be measured or calculated ahead of time. Because of the very rapid growth of gamma-ray intensity, it was clear that unless an extremely fast cable was used and, furthermore, was radial to the bomb, the cable would be shorted before the signal from the detector could get to the recorder. This fact then governed the required output of a detector and made it clear that many amperes of current were necessary. From these criteria, Rossi designed a system involving an ion chamber some six feet long and six inches in diameter as a detector, and transmission cable that was three-inch diameter copper coax, one-inch diameter inner conductor, air dielectric, that was run on catenaries from the tower cab to the ground and then buried in trenches the rest of its way to the recording station. He made a loop some 300 feet in length of this three-inch coax just outside the station to be able to tap off the beginning of the loop to operate the scope intensifiers and then let the signal go the extra 300 feet before being presented on the scope so that the intensifiers would have time to work. He furthermore originated the "Rossi Presentation," which involved a constantly oscillating sine wave with appropriate frequency (190 megacycles/sec) on the vertical plates of an oscilloscope and the signal on the horizontal plates. Thus, no matter how fast the signal, there should be an initial portion that, by the very characteristic of an exponential, moves slowly enough for a few cycles of the oscillator to be presented. Rossi's system worked on Trinity and produced a trace (See Figure 5) that was very fuzzy but, nevertheless, did show the reaction rate such that it could be measured with a probable error of approximately 10 percent. [redacted]

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Figure 5.

Rossi presentation from Trinity. The ionization chamber output signal is oriented vertically and the fixed frequency oscillator sweeps horizontally in the figure. The three arrows mark the extrema for one cycle of the oscillator. The value of alpha is computed from the signal amplitudes at the extrema relative to an arbitrary baseline.

In parallel with this effort, Bob Wilson developed a unique presentation system that consisted of a charge collection box built into the face of an oscilloscope. By allowing a sweep only in the horizontal direction, starting to one side of his charge collection box, he could arrange a geometry in which the charge collected on the box was related directly to alpha. Unfortunately, because oscilloscopes tend to change their characteristics with time, this method required that a calibration signal be measured approximately minus one second from the detonation. At Trinity, the calibration system failed to operate so only the real measurement was made. Since there was no calibration, it could be interpreted only from calibrations taken many hours before. The result he obtained [redacted] was correct, but he could not state the uncertainties. Therefore, in later operations, the designers of new systems followed on from the Rossi system rather than the Wilson system. With the exception of a small amount of work by Clarence Jones during Teapot, to the best of my knowledge, no one pursued Wilson's ingenious path after that.

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For Sandstone in 1948 Los Alamos set up a temporary division to conduct the technical portions of the operations. The Laboratory requested and accepted the assistance of competent outside laboratories and, on the subject of reaction history, specifically that of NRL under Wayne Hall and Ernie Krause and of EG&G. Technical liaison and direction on this subject was in the hands of the author and Gus Linenberger. The successful measurement on Trinity having been the Rossi measurement, it was decided to follow that path and make what improvements could be made. No personnel were left in the Laboratory after the great exodus of 1946 who had been deeply involved in the Trinity measurements. The immediate path was to discuss the subject again with Rossi. Those discussions, plus Los Alamos and NRL thoughts on the subject, did not lead to any deep further understanding immediately. NRL could and did make improvements on the oscilloscopes available at that time and produced higher writing speed oscilloscopes. However, no significant advance in the understanding of cable transmission was achieved. Therefore, the basic Sandstone alpha measurements were essentially a repeat of those made at Trinity. Fortunately, two of the Trinity detectors were still on hand and from those, plus drawings, a new stable of identical detectors could be made. The same coaxial system was used that Rossi had on Trinity, that is, three-inch coax, buried after the catenary, some seven feet deep in the coral sand. Also the delay loop and turning philosophy was the same as Rossi had used. The only real difference between Rossi's measurements on Trinity and those conducted on Sandstone was the number of detectors, cables, and oscilloscopes. Sufficient detectors to cover the entire expected range that was detectable were used. The measurements, under the excellent direction of Wayne Hall and especially Ernie Krause, were successful.

However, the large expense and effort involved in this led to a suggestion of another technique by Edward Teller. Edward made the comparatively straightforward observation that gamma rays passing through air produce secondary electrons. Those electrons excite atoms and molecules and those atoms and molecules, in falling back to the ground state, produce light. Since all of the processes followed the initial exponential, and exponentials of the same value added together produced the same exponential, it was obvious that observation of the rate of growth of the light intensity at very early stages gives alpha. Again, it was assumed that the gamma-ray intensity followed the neutron intensity properly. Both Edward and the experimenters to carry this out, namely the author and EG&G, went through the appropriate arithmetic and calculated that the light intensity would be observable. Edgerton, Germeshausen, and Grier were given the job of actually conducting the measurement. The field effort was fairly straightforward. Both photocells and photomultipliers, specifically 930 photocells and 931A photomultipliers, would be used at the focus point of five-foot mirrors, salvaged from Army arc light searchlights, and placed several miles from the detonation as detectors. The signals from these detectors were limited to comparatively small currents so it was necessary to use some electronic trickery to drive the then existing oscilloscopes. Both amplifiers and very high impedance circuitry were used. At the same time that EG&G was developing the equipment to make the measurement, the workers at Los Alamos set out to measure in the Laboratory the actual conversion efficiency from gamma rays to light. In order to do this they used photomultiplier detectors in a steady-state experiment involving a contained air volume in a black enclosure and gamma rays from the Laboratory's approximately 10,000-curie radiolanthanum source. If the conversion efficiency was as calculated, the observation should be straightforward. Basically, one simply varied the air pressure in the container and observed the light output. The output should vary with the air pressure, and any background effects could be separated by observing the light intensity with no air in the container. Unfortunately, the results of this laboratory experiment showed that the conversion efficiency was something of the order of a factor of 1,000 less than the arithmetic had indicated. A meeting just before the field teams disappeared to the Pacific again illustrated Teller's magnificent physical intuition. After a number of hours of argument, Edward simply observed that we the experimenters were ready to make the measurement and were on the way to the field, so why not make it anyway. So they did. The field measurements indicated that the light curve did follow alpha within the observational range. They also showed that a growth rate of higher than one-half generation per shake could not be measured with 931As, but higher alphas were obtainable with the 930 photocells. Later on, the answer to the initial dilemma became clear. While the conversion efficiency from gamma rays to light was a factor of 1,000 down from the initial calculations, the conversion efficiency from neutrons to gammas in the bomb and, hence, the gamma ray output at a given neutron level in the bomb, was a factor of 1,000 more than the calculations had indicated. Thus, the light intensity observed was very close to that shown in the initial arithmetic.

Sandstone also saw the beginning of serious studies of the electromagnetic pulse produced by a nuclear detonation. That pulse caused great trouble in normal observations because it was picked up on signal cables, etc., and distorted the signal that was intended to be measured. The observation of this phenomenon, of course, led to a later method of observing alpha. The existence of such a signal had been predicted by Fermi and noted by Bob Wilson on Trinity.

The establishment of a permanent test division at Los Alamos, J-Division, in 1949, with the appropriate people to study the problem, plus the recognition that thermonuclear reactions, [redacted]

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Thus, the NRL group (under Krause) and EG&G, with appropriate funding, developed new high writing-speed oscilloscopes, including the traveling wave oscilloscope, that were eventually to reach writing speeds as high as the velocity of light. They developed very high-power, fast amplifiers in order to look at lower levels in the alpha signal. They studied, but did not particularly improve, cable propagation and they developed new detectors. [redacted]

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To do this, NRL and EG&G, in conjunction with Earl Fullman of the LASL, developed photocell-phosphor combinations with high sensitivity and short integrating times. The photocells were especially built photocells some three inches in diameter and 10 inches long, which in practice were then enclosed by a phosphor, on the order of two-inches thick, which was initially naphthalene, but later some of the liquid phosphors. Because of the conservatism of the Los Alamos contingent, it was necessary to use both the photocell systems and the old ion chamber systems until we had determined that the photocells actually would give the same results where appropriate. An initial check was run by Bob Patten on Ranger. The photocell-phosphor combination had received some impetus during Sandstone from a variation on the Teller alpha measurements, in which large, flat plates of naphthalene were put up close to the bomb and the light from those plates observed using 930 photocells with an appropriate mirror focusing system at some distance from the bomb.

The basic techniques developed for Greenhouse for the measurement of alpha became the standard techniques through the period 1951 through 1958 with comparatively minor variations. New photocells were developed for slightly faster response. Oscilloscope research continued and produced ever more satisfactory oscilloscopes. Trigger circuits were developed that allowed shorter delay loops and perhaps most important, the understanding of cable construction and transmission grew so that it eventually became possible to use somewhat smaller cable (7/8-inch diameter) by correcting for attenuation at the high frequencies.

The sudden necessity to conduct the Ranger operation around Christmas of 1950 and early 1951, before the Greenhouse operation, led to an unexpected, but in the long run, quite profitable variation of the use of the alpha system--the alpha measurements being under the guidance of R. B. Patten. This was the first time since Trinity that LASL had conducted, in the field, all of its own alpha measurements. The basic gear that had been developed for Greenhouse was used along with some old Sandstone equipment. Specifically, the ion chambers developed by Rossi, which were to be used again at Greenhouse, were used as detectors. Since the bombs were to be detonated at something like 1,000 to 1,500 feet, it was recognized that only late-time measurements were possible, but, since these bombs presumably had a constant alpha, that was satisfactory. A single blockhouse was constructed that would contain some 25 to 30 oscilloscopes and four sets of detectors were placed at the ends of a cross centered on the recording building on the order of 500 feet out from the building. Delay loops were wound and installed inside the building. The whole array then became the target for the bomber. It was the philosophy that if he missed the building, he would hit moderately close to one of the detector systems at the end of one of the arms of the cross. This system worked adequately with comparatively minor problems from a technical point of view* and led to the use of similar systems for airdrop and balloon shots at the Nevada Test Site during later operations.

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*However, there were serious operational problems. The shock wave from the detonation hitting the ground caused the ground to fluff so that eventually there was soft sand of the order of eight feet deep around the building through which it was almost impossible to walk and vehicular traffic was not possible. In between shots, it was necessary for the people to get into the building, which had a long entrance tunnel, and they had to go through this fluff which was, of course, quite radioactive because of activation from the neutron flux from the devices. They had to pass through this radioactive region and into the shelter of the building quite rapidly to check out and reset their system. Getting to the detectors became most difficult because of the radioactivity. As the operation went on at approximately one shot per day, the detectors were gradually destroyed and for the last shot could not be replaced without overdosing personnel appreciably. To solve this problem, the field team at Frenchman Flat, namely Jack Clark and Bill Ogle, simply moved the lighted target array to the one set of detectors that was still operating properly, in order to increase the probability of getting a signal. Since the bombers were bombing on the lighted array, it did not occur to the field team that anyone else could possibly care about this movement, so no notice was given to the Air Force or the Test Manager and Scientific Advisor, who for Ranger were in Las Vegas. The last bomb, Ranger F, was dropped successfully and the alpha measurements were achieved successfully. However, some three days after that shot, the reporters, in a normal press briefing, inquired of Al Graves, who was the Scientific Advisor, as to whether the target had been

The attempts to bring Teller alpha into the status of a dependable measurement were continued in Greenhouse to a certain extent but with no real breakthroughs. It did not seem that one could make the photomultipliers respond at higher rates than an alpha of perhaps one generation per shake. The use of ordinary photocells to observe fast alphas, such as those expected with boosts, did not seem feasible because of the requirement for very fast amplifiers, which were not yet available. However, EG&G, Wayne Hall at NRL, and groups at Los Alamos continued to work on this subject.

[redacted] The alpha measurements were therefore solely Teller light measurements, conducted by Lee Aamodt, using 930 photocells and five-foot dishes to collect the light. The measurements were adequate and successful. Between 1954 and 1958, there were continued efforts on the development of Teller alpha. However, in that period, physically smaller single-stage devices were developed. [redacted] It was recognized that the light signal, which was far down the chain from the neutrons, would not follow the neutron intensity in the device with high accuracy and our capability of calculating back from the light intensity to the neutron density involved so many steps that it could not be done accurately. Therefore, the measurement was not considered a principal diagnostic measurement up through 1958. It was, of course, pursued at that time and later as a possible observational technique to use in observing foreign tests.

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Beginning in approximately 1948 (though predicted by Fermi before Trinity), the testing system recognized that in the same fashion as Teller light, the bomb should produce an electromagnetic signal, the early stages of which, for the same reasons, should, in principle, follow the alpha curve. In this period of time, a number of experimenters investigated this phenomenon in great detail and tried to convert the measurement of this phenomenon into an adequate measure of alpha. Ernie Krause, during Greenhouse and Sandstone, devoted some of his oscilloscopes to this effort. Watt, Malik, and Theobald at Los Alamos continued to investigate portions of the problem. Watt, specifically, tried to look at the field inside the high-intensity gamma sphere. Lou Wouters, at UCRL and later at Livermore, conducted both measurements and theoretical investigations of this phenomenon. Bob England and Clyde Cowan at Los Alamos conducted rather large experiments during Buster as did Ralph Partridge on Tumbler-Snapper. England and Partridge were the first to show that the light curve and the electromagnetic curve, at least in the early stages, followed the proper exponential. Unfortunately, these results were not well recognized and it is only in very recent years (1973), due to increased capability, that we have been able to again experimentally arrive at this conclusion. Thus, at the beginning of the moratorium in 1958, the measurement of reaction history by observing the electromagnetic signal was not in a satisfactory state. The measurement of long-time electromagnetic signals out to many microseconds had been conducted largely by

*(cont)moved. He commented, "No," and a few hours later asked Ogle the reason for the question. The answer was that it had been moved but notice of that fact had not been considered important. Graves was extremely embarrassed and from then on, rejected the philosophy that the Test Manager and Scientific Advisor could be physically separated from the rest of the technical organization in conducting an operation.

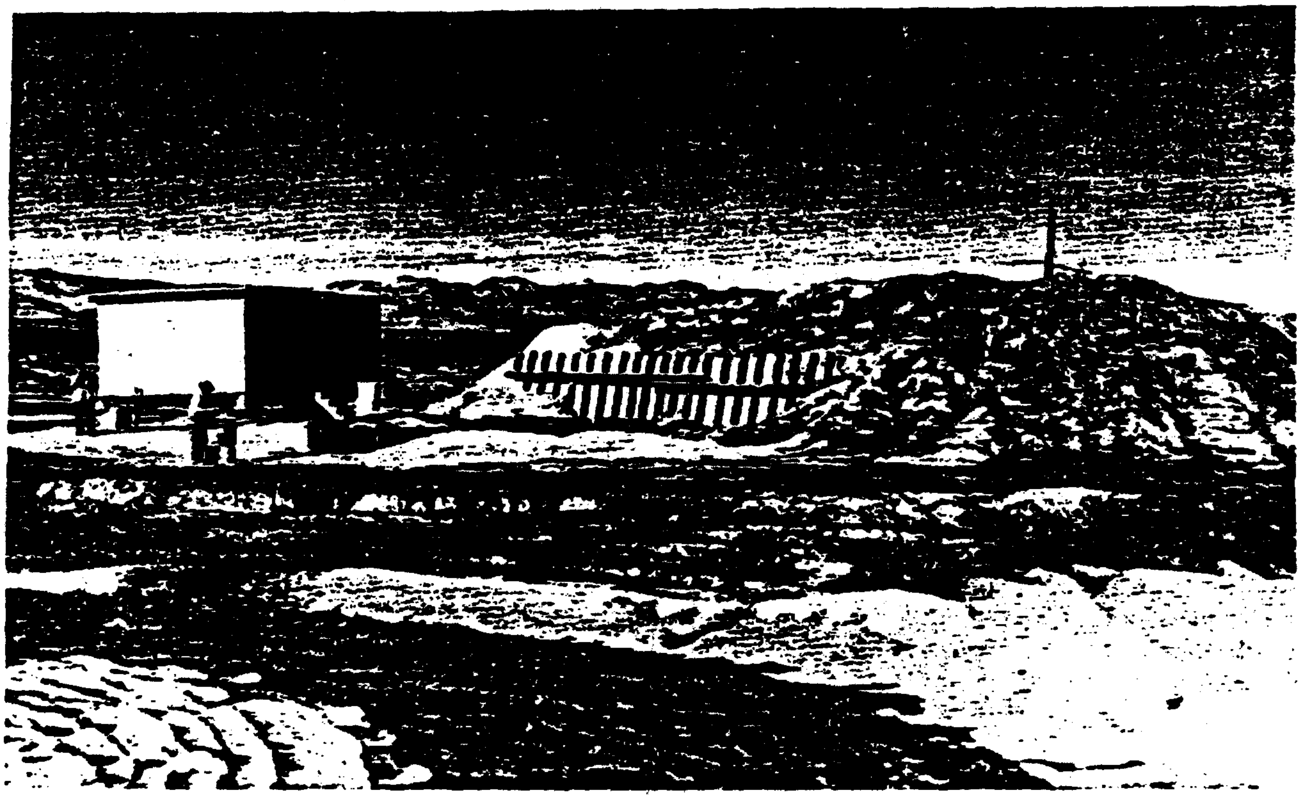

Figure 6.
Ranger alpha recording building (under the pile of dirt). The small building was removed for the detonations.

contractors to the Department of Defense for entirely different purposes and will be discussed in a later section.

As previously mentioned, the advent of the thermonuclear reaction, or boosting, at Greenhouse in 1951 led to the requirement for much faster scope writing rates, better detector response, and better cable transmission characteristics. The developments previously mentioned, largely those by the NRL group under Krause, led to the desired capability. While the reaction rates were also measured by other techniques, the simple technique of following the gamma-ray curve worked satisfactorily on the first boosted device (Greenhouse Item) [redacted] To do this properly, of course, required not only the assumption of the inverse square law, but a knowledge of the attenuation of air for the spectrum of gamma rays produced. This knowledge was obtained both by common timing between detectors at various distances and by other measurements of the gamma-ray intensity as a function of distance.

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Bhangmeter

The optical observations at Trinity in 1945, both by camera and other instruments, showed a double-peaked illumination curve for the light from the bomb. Very early calculations on the fireball expansion phenomenon also indicated that there should be two peaks to the light curve with a minimum of intensity after the first maximum coming at about breakaway, that is, at the time the shock wave breaks away from the expanding front of the fireball. This phenomenon takes place presumably because of the cooling of the fireball front as it expands and because of the formation of nitrogen, oxygen, and hydrogen compounds in the high-temperature shock front before breakaway, and because the opacity of heated air to visible radiation is sufficient to cause absorption of the light from the inner glowing hot gas. As the shock front cools it gradually becomes transparent, allowing visible radiation to escape from the inner hot regions, resulting in an increase in thermal radiation, and producing a minimum in the light curve. The time at which the shock front begins to be transparent is yield dependent.

During Sandstone, as an afterthought, a very simple measurement of the light intensity vs. time was made using a photocell driving the horizontal plates of a cheap oscilloscope. Timing was established by means of a 1,000 cycle per second signal impressed on the vertical plates. The simplicity of the Sandstone measurement technique led to the suggestion, by Fred Reines, after the formation of a permanent test division at Los Alamos, that a simple instrument designed solely to allow a quick observation of the time to the minimum might prove valuable operationally and could conceivably, in the long run, be a dependable method of yield measurement. LASL, therefore, requested that EG&G construct such an instrument and produce several in a portable form. In short order, EG&G designed and constructed a prototype basically consisting of a 930 photocell (blue sensitive surface) and appropriate circuitry to present the signal on a small oscilloscope, which had timing markers on the sweep. Appropriate expanding and compressing circuitry was arranged so that the signal would remain on the oscilloscope face. The scope was then photographed with a Polaroid camera, so that a reading could be obtained within a couple of minutes after detonation. It was common to use four or five such instruments on a detonation. The time to the minimum was then read by several different observers and the numbers averaged out to pick an official value, from which the yield was then estimated.*

Various studies in LASL, EG&G, and the Department of Defense on the theory of the minimum in the light intensity gave somewhat different exponents for the scaling law, usually not one-third. It quickly became apparent, as a result of the more detailed measurements of the light curve by NRL, that the time to the minimum varied with the color of the light observed, but the official bhangmeter continued to use a blue sensitive surface, since that was the surface that had been calibrated. The time to the minimum was affected to a certain extent by the surroundings of the

*The author always arranged to have a bhangmeter of his own during the operations in order to get his own time to the minimum and woe be to EG&G if their official number was appreciably different than his. An intense afternoon was spent by the entire Group J-7, with its group leader Fred Reines, early in 1950, picking a name for this world-shaking device that was going to produce simple, cheap, and easy yield measurements. At the end of the afternoon, Reines picked a name which we all knew would be misinterpreted for the rest of history. Bhangmeter is not synonymous with bangmeter. Bhang is a variation of Indian hemp, the leaves and seed capsules of which are chewed or smoked, and which then produces the same euphoria as other variations of hashish. The now obvious connotation is that we were off our rockers to think that this thing would ever be particularly useful and anyone else who ever believed it must also have a little something wrong with them.

device when it was fired. A tower shot, with appreciable mass in the tower, might give an answer a little different than an airdrop. A surface shot could give a strong difference because the fireball was expanding in a hemisphere instead of a sphere. In fact, if the shot is at the surface of a perfectly reflecting plane, the surface of the expanding hemisphere follows essentially the same time history as that of an airburst of twice the yield. Since the numbers were very simple to treat, and were available to everyone who happened to be around when the shot was fired, a great number of people had their own calibration curves, which differed enough to lead to great and heated discussion, the difference usually coming about from slightly different interpretations as to the time of the minimum or different yields used for their calibration shots (for example, using fireball yield instead of radiochemical yield, or vice versa) or different personal corrections for the manner of firing. During the period 1950 to 1958, EG&G constructed several more sophisticated versions of the bhangmeter, but they all operated on the same principle. Very late in the game, a few bhangmeters were built with different photosensitive surfaces having different spectral characteristics. The bhangmeter did serve its purpose admirably. By the end of 1958, it was considered to be an instrument that would give the yield (most of the time) to plus or minus 15 percent, and it did have the advantage that it could work off reflected light at an appreciable distance. Thus, by the time of the moratorium, this was a mature tool for the determination of the yield of fairly low-altitude detonations, that is, well into the atmosphere. The author's personal calibration curve, as a result of the experience through 1958, is shown in Figure 7. The bhangmeter could be used on the Dominic airdrops, but obviously was of no value for underground detonations.

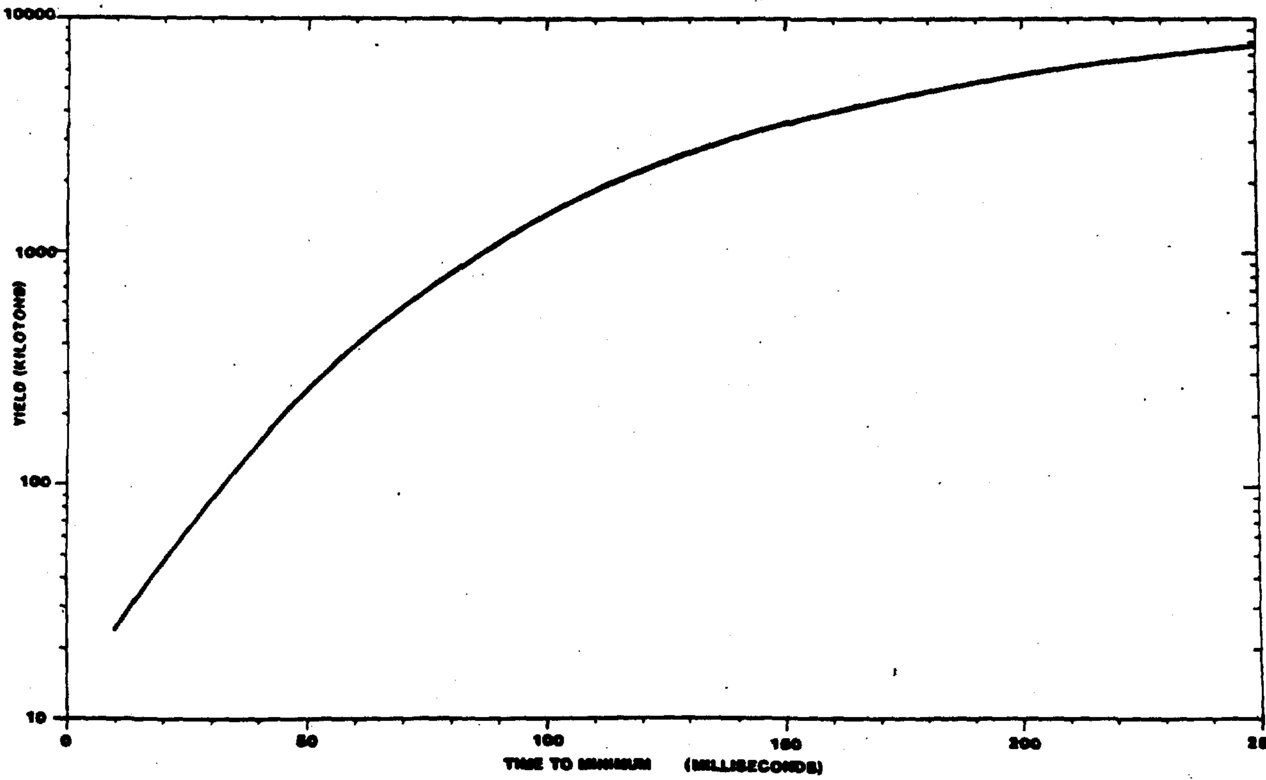

Figure 7.
Author's bhangmeter curve for high yields (1958). Yield vs time to the minimum.

Time Interval

Beginning with the first two-stage device, Mike, in 1952, it became necessary to measure another diagnostically critical number, the time between the primary detonation and the detonation of a secondary. Having developed the primary tools to measure alpha, this was in principle fairly straightforward. But a few words on the subject are, perhaps, in order.

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In fact, Aamodt and others on Greenhouse had observed the electromagnetic signal from single-stage devices at an appreciable distance from Eniwetok, having stations on Kwajalein, Guam etc. Sandia, in the early Nevada operations after Ranger, put stations to observe both light and electromagnetic signals as far away as Albuquerque. In general, it was observed that the time interval could be measured by the electromagnetic technique up to roughly 500 miles* from the detonation over a sea surface. The equipment for so doing was simple, consisting of antennas feeding directly into comparatively fast oscilloscopes, generally with amplifiers. Recognizing this simplicity, time intervals were measured at Castle largely by a single electromagnetic station on Japtan (operated by Rod Ray and John Malik**) at Eniwetok, which observed the time intervals both from Eniwetok and Bikini shots. [redacted] ~~measurement was also made by other techniques. From then on both in Nevada and at~~

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*Glen Jean, National Bureau of Standards (NBS)--Bikini from Wotje during Castle.

**The electromagnetic time interval experiment was actually designed and fielded by Bob England and Ray, but England died a few days before the beginning of Castle in a laboratory accident at Bikini. Thereafter Malik was the project leader.

the Eniwetok Proving Ground, the time interval on two-stage devices was measured by whatever technique, or combination of techniques, seemed to be the easiest at the time. If there were close-in alpha measurements or gamma-ray intensity measurements, then it was simple to observe time interval by observing the gamma signal. If that was not convenient, for example, on the airdrop Cherokee or on some of the barge shots, then Teller light or electromagnetic signals were used. All of these techniques were well developed by 1958. However, it is notable that there was not very much experience, at least in the AEC family, in making these measurements for bombs dropped over water such as we eventually did in Dominic and, hence, the question of reflection of the electromagnetic signal off the water surface had not been seriously considered. [redacted]

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AEC Device Diagnostic Nonstandard Measurements

We will now briefly mention some other types of diagnostic measurements that were developed during the period 1950 to 1958, not because they were critical in the return to testing in 1961, but rather to illustrate the kind of information that, in principle, was available but in practice could not be obtained from airdrops, as in Dominic, or initially from underground shots, as in Nevada. Only in recent times have some of these types of measurements been possible in Nevada, and some of them have not yet been reproduced.

Dinex

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[redacted] The separation of specific energy protons then allowed the observation of a batch of particles, all of which had the same flight time from the burning region to the detector; and, hence, the time smear in the neutron signal due to different flight times was obviated. Protons then impinged directly on an appropriate collecting cup and the resultant signal was sent through cables to fast oscilloscopes at the recording station. The practicalities of the experiment involved such large amounts of materials, both in magnets and in lead to shield the cables. [redacted] In fact, hunks of melted lead were picked up years afterwards, on Aoman-Biijiri, as a result of that shot. The experiment cost on the order of 10,000,000 in 1951 dollars.

Ganex

To make the same kind of observation as Dinex, but somewhat less expensively, an experiment [redacted] was designed which involved a large iron converter [redacted] At the converter the neutrons, through the (n,γ) reaction in iron, produced gamma rays. The observation of those gamma rays at a comparatively remote point, through systems collimated on the converter, allowed an observation of the neutron burn rate. This technique was not used very often after Greenhouse because of the observation that the boost signal could be observed by normal alpha techniques, except in unusual circumstances. Variations of the technique have been used underground in recent years.

Thermonuclear Burn Propagation Rate

On Castle Bravo [redacted] in 1954, Sterling Colgate and co-workers of UCRL performed a classic experiment in which they measured the burn propagation [redacted] In concept this massive experiment was comparatively simple but difficult of execution. The basic experiment consisted of collimators of the appropriate material very close to the device, that is, just outside the point which the case would reach before the secondary exploded. The neutrons from the burning secondary then passed down an array of 2,500-yard long vacuum pipes, approximately six inches in diameter, to fast detectors in a building at the far end.* The detectors converted the neutron signal to an electrical signal, which was then recorded on the oscilloscopes in the next room. Of course, it had to be shown that the cross talk from channel to channel could be kept to satisfactorily low levels. Since the propagation rate was extremely high and the burn rates were high, the most modern detection and recording procedures had to be used. This experiment, which worked very well, and other similar ones in later years, led to a better understanding of the burn propagation through thermonuclear materials, [redacted]

*Krause (NRL) had performed a similar measurement on Ivy Mike using a helium-filled tunnel instead of vacuum pipes.

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Tenex

[redacted] The measurement of radiation temperature was attempted by observation of the x-ray spectrum. It was also recognized that the spectrum of the 14-Mev neutrons produced by the DT reaction in the burning region would be broadened due to the very nature of the thermonuclear reaction. That is, the DT reaction takes place because of high thermal motion of deuterons and tritons reacting upon collision. The neutron from the reaction has roughly 14-Mev energy in the center of gravity system of the deuteron and triton, but since that center of gravity is moving with respect to the laboratory system, the neutron will have varying energies in the laboratory system depending upon the motion of the center of gravity. The widening of the spectrum due to this phenomenon is easily calculable for any given burn temperature. Therefore, an observation of the detailed spectrum around 14-Mev would, in principle, allow a determination of the particle temperature. The measurement of the x-ray spectrum, if successful, would give the radiation temperature. It was recognized that it is possible to have a burn in which the radiation temperature and particle temperature are not the same, so both measurements were of interest. Since the neutrons are particles and travel at appreciably less than the velocity of light, even at 14-Mev, and their velocity varies with energy in a well-known fashion, it became clear that an observation of the time of arrival of the neutrons at some point distant from the bomb would allow a detailed measurement of the spectrum near 14-Mev. Experimental criteria were straightforward. The detector had to be at such a distance that the time spread between the arrival of the lowest-energy neutron expected and the arrival of the highest-energy neutron expected was long compared to the burn

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time of the thermonuclear reaction. A measurement of the spectrum between 12-Mev and 16-Mev would be adequate to determine the temperature, although in actual practice, the spectrum was measured over a somewhat wider band. [redacted] Since the measurement would be simply the observation of the current from a detector, it was clear that the detector sensitivity as a function of energy was required and this quantity could be both calculated and measured in the laboratory. There was a little trickiness involved in setting up the oscilloscopes so the sweep would be on at the time of arrival of this band of neutrons. However, by triggering off the rise of the gamma signal (alpha), this problem was solved. Such measurements were conducted by Hall and Waddell [redacted] and operated satisfactorily. The detector stations were at approximately 200 and 1,000 yards from the device. [redacted] Variations of this Tenex technique were used both in Nevada and in the Pacific during most of the operations up through Hardtack. (Variations are now used underground; however, the experiment is in some ways difficult because of the comparatively short distance that the detector can be from the device.) Later theoretical calculations showed that the broadening of the 14-Mev spectrum could come about for reasons other than simply temperature broadening. The deuterium and tritium mass could be moving as a body one way or another, which would only produce a shift in the peak; but if different parts of the burning region were moving in different directions, the spectrum would appear to be broadened. The effect of this additional broadening could be treated theoretically. This diagnostic technique had reached moderate maturity by 1958.

Pinex

The use of threshold detectors led to a design of another fairly valuable diagnostic tool, but one which produced data that was perhaps more of wonder in the period before 1958 than of actual use to the theoretician, mainly because the computer codes of that time were not sufficiently developed to take account of the phenomenon observed. This measurement was called Pinex and simply consisted of a neutron camera using the high-energy neutrons, that is, 14-Mev neutrons from the thermonuclear burn region, to carry the image. A steel collimator placed some distance from the bomb furnished the pinhole of the neutron pinhole camera. At an approximately equal distance back of the collimator, a plate made of an appropriate threshold detector, initially zirconium, was placed. Upon detonation, the high-energy neutrons from the thermonuclear burn region of a bomb passed through the collimator and pinhole and formed an image on the zirconium plate of the same shape as the burn region and with an intensity related in some way to the burn in that region. Thus, a picture of the integral burn of the booster region, as shown by the 14-Mev neutrons, could be obtained. After exposure, the zirconium plate was recovered, taken back to the laboratory, sliced into very small bits, and their induced radioactivity measured. From that data, a mosaic could be built up to get a picture of the source. Later on, it became obvious that one could simply lay a piece of photographic film on the zirconium and get an image directly from the zirconium activity. Appreciably later, by shielding against the gamma rays, short half-life materials, such as aluminum, were used. Photographic film was placed against the aluminum preshot. High energy neutrons from the explosion induced radioactivity in the aluminum resulting in exposure of the film shortly after the explosion. The film could then be recovered, developed, and would give directly an image of the burn region. This technique, originally developed for tower shots in Nevada, was eventually developed for use both on primaries, and on secondaries on barge shots, even of megaton devices, since the camera could be protected by the water and recovered from the bottom of the lagoon.

By 1958, the technique was well developed and satisfactory for atmospheric detonations. However, again, we had no experience in using the technique underground or on airdrops. Parenthetically, one may note that after several years of underground testing, variations of this technique have become quite important diagnostically. The Laboratories have now developed techniques for either recovering the image recording material from downhole or producing images through the use of fluors in image-transmitting systems which can then retransmit the image uphole. But only in quite recent years have the data obtained by this method been actually useful in a calculational way to the theoretician, because only in recent years has the calculational capability been developed to handle the problem.

Output Measurements

Another class of measurements are on the borderline between effects measurements and diagnostics measurements. They were useful on both sides of the house. Except for Trinity, the DOD laboratories did not contribute appreciably in these fields until in the mid-1950s when AFSWP began to develop significant in-house competence in the field.

Neutrons

At Trinity, Klema exposed samples of sulfur and gold (shielded and not shielded by cadmium) to the neutron flux from the Trinity device and observed the induced radioactivity. Calibration of the particular sample geometries used on laboratory sources, such as the Omega reactor and the Van de Graaff accelerator, allowed a translation of these data into numerical quantities for the integral neutron flux as a function of distance from that device and an initial attempt to determine the spectrum. These data were very valuable to the early weapons effects philosophers. When Crossroads was planned in 1946, the methods of measuring yield were still somewhat uncertain, and it was felt worthwhile to repeat this simple measurement as one of the many attempts to compare the yield of the Trinity device with that of the supposedly identical follow-on device to be dropped in Crossroads. At that time, there was no particular conviction on the part of the weapons designers that two devices, built the same, would actually operate the same. The uncertainty had to do with the question of when the first chain reaction would actually start, an uncertainty, incidentally, that was to plague designers many times in later designs. The particular counters, sample molds, and calibration sources that were used on Trinity were found, and hence, the identical measurement could be conducted. In addition to the device uncertainty, there was some question as to whether or not the spectrum would change as a function of distance because of the reflecting characteristics of the water surface, as opposed to that of the silica sand of the Trinity site. With appreciable operational difficulty and high adventure on the part of the experimenters, the measurement was repeated on Crossroads Able and indicated that the yields of the two devices were the same within experimental error and that there was no appreciable effect of the water, probably because the Crossroads Able device was fired at moderately high altitude above the water. As a side benefit of the experiment, it was also possible to show that the bomb had missed its intended detonation point by approximately 700 yards.

When the planning for Operation Sandstone came along in 1947, it was again decided to repeat this measurement, probably for no awfully good reason except the enthusiasm of the experimenters involved. However, since the devices were of different construction, with those for Sandstone using smaller high explosive, it was to

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be expected that the neutron spectrum would differ to some extent. Furthermore, since the Sandstone shots were on towers, it was possible to measure the flux and spectrum with somewhat more accuracy than was possible at Crossroads and also to acquire some data on the variation with yield. For these experiments sample lines were placed both along the land and over the water surface and the results did show some difference in flux and spectrum over the two surfaces, especially in the slow neutron range as detected by gold. The actual neutron intensities as measured above the 3-Mev sulfur threshold were very nearly proportional to the yields of the devices. The fast neutrons as detected by sulfur showed an almost pure exponential drop-off with distance, after the inverse square effect was taken out, which was to be expected, but the slow neutrons showed a pile-up close to the source and extending out perhaps 200 or 300 yards. After that, the slow neutron intensity fell off essentially exponentially following the same curve as the high-energy neutrons, indicating that the far-out slow neutrons had gotten there as fast neutrons. All of this helped the understanding of neutron propagation through the air, which at that time was still under some debate theoretically.

The expectation, after Crossroads, that thermonuclear reactions would someday be attempted, led to further concentration on the part of the experimenters as to how these techniques could be used to further diagnose the devices. The expectation of a thermonuclear burn [redacted] made it necessary to attempt to measure the amount of burn. External threshold detectors were an obvious technique. In the period between [redacted] a laboratory investigation using the high-energy gamma rays from the betatron led to several possible new detectors, the most outstanding of which, because of its convenient half-life, was zirconium. Zirconium could be used in the field as a (n,2n) detector with the threshold at about 12-Mev. In the laboratory, that threshold could be measured using the (γ,n) reaction, the gamma rays coming from the betatron. Obviously, an external measurement with detectors at some distance from the bomb also required information on the attenuation due to air over the distance from the device to the detector and the attenuation from the source inside the device to the outside of the device. [redacted] with adequate success. Another technique for measuring the overall burn was, of course, internal detectors which were then collected as part of the cloud and treated radiochemically. These two techniques, that is, both internal and external detectors, were then used through the rest of the period up through 1958 to determine the burn of the primary boost region.

Iodine, with a threshold (n,2n) reaction at roughly 9-1/2 Mev, was used on Sandstone in order to get a background calibration to see if this threshold detector would be satisfactory to observe the high-energy neutrons from the thermonuclear reaction that we could suspect was coming on some later operation. Iodine has a decay half-life of 13 days, which made moderately prompt recovery and counting important. In its use, it was necessary to use both unshielded and lead shielded detectors in order to separate out the activity induced by very high-energy gamma rays of the bomb. Neutrons coming out of the bomb and being captured in the nitrogen of air result in approximately 10-Mev gamma rays of very long mean free path, which had to be dealt with as a background.

In parallel with the above-mentioned effort [redacted] Louis Rosen developed a technique to measure the spectrum of neutrons above, perhaps, 1/4-Mev. This

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technique involved the use of stations at various distances from the bomb with small-diameter neutron collimators many feet thick, behind which was placed a "neutron camera." The neutron camera consisted of a hydrogenous scatterer, which then emitted protons resulting from (n,p) scattering in the scatterer. The protons were recorded on nuclear emulsion photographic plates and produced tracks of measureable length in the very thick emulsion. Rosen had been using a similar technique in the laboratory and, hence, had done a great deal of work on the track lengths as a function of energy in the nuclear emulsion plates. These cameras were then collected and sent back to the laboratory, where the plates were developed and read by a great team that Rosen had at his command. This was an eminently successful technique for determining the spectrum coming from the bomb in a moderately straight line; but since it was well collimated, it had less value from an effects point of view because it did not, in general, measure the scattered neutrons, i.e., those that were not coming radially from the bomb. Obviously, corrections to obtain the total neutron flux could be made. This method of obtaining the neutron spectrum was comparatively expensive, but was nevertheless used by both weapons laboratories on the appropriate occasion throughout the remainder of the period under discussion. The detailed spectrum obtained was of appreciable value in checking the corresponding neutron output and transport calculations.

One other neutron flux measuring technique deserves to be mentioned, the so-called fission-foil camera. This device, starting with Greenhouse, collected the fission fragments emitted from plates of uranium-238 or -235, shielded or not shielded by lead or cadmium, on a rapidly moving cellophane film. The cellophane film could then be cut up into small strips and their radioactivity measured in a laboratory counter to determine the neutron flux as a function of time after the detonation. Perhaps the major pertinent point that came out of the use of this technique was simply that there was a burst in the slow neutron flux as the shock wave passed the camera.

Obviously, the total neutron output from thermonuclear burn regions could also be obtained from the reaction history experiments. However, in general, the absolute calibration of the detectors and electronics used in those experiments at that time was not sufficiently good to allow an accurate integral measurement.

Gamma-Ray Flux

The total gamma-ray incident radiation at a distance from nuclear detonations is composed of several parts. One is the prompt radiation from the device itself during its multiplying or immediate disassembly stages. Another is the radiation from the rapidly decaying fission fragments or other activated nuclei as they mix and rise in the fireball, eventually, to form the moderately stable radioactive cloud. There is, on occasion, some contribution at ground level from the stable cloud itself; however, this is usually small because of the great attenuation of the air between the cloud and the ground. Another contribution comes from the capture of neutrons in air and subsequent decay of the resulting nuclei with gamma emission. No appreciable contribution is due to the x-rays from the fireball at distances of interest because of the extremely short mean free path of x-rays in air.

Straightforward techniques for observing gamma-ray dose had been developed over the years before Trinity for use in laboratory medical installations, etc. These techniques were used in the field at Trinity and Crossroads, where film badges and dosimeters were spread with great profusion over the area around the device. After exposure they were collected, developed, and read in the laboratory in the same fashion as any other film badge. Early-on, various shielding materials were used in conjunction with unshielded film badges and dosimeters to allow correction for the neutron dose to the film badge (the neutrons scattering in the hydrogenous emulsion produced protons which, in their slowing down, cause ionization resulting in darkening of the film). The problem of equilibrium in a hydrogenous mass, such as the human body, was recognized, so that appropriate mock-ups were made to help translate the simple observations into whole body dose. Over the years, better and better process control was established to allow more precise measurements. Early measurements showing the variation of dose with yield were made by Pete Scoville at Sandstone, but after that time, the effort was largely carried out by Ellery Storm of H-Division in LASL, and H. O. Wycott and L. S. Taylor of the National Bureau of Standards, with the assistance and guidance of John Malik. It was quickly observed that the gamma dose was, for a given device design, closely proportional to the fission yield.

More sophisticated measurements were attempted, beginning with Greenhouse, to understand the production, transport, and deposition of gamma rays. At Greenhouse in 1951, the National Bureau of Standards attempted a detailed observation of the gamma rays from the radioactive cloud in the very early stages of fireball expansion and cloud rise by means of a massive station fairly close in, with a great number of collimators pointed in different directions and magnetic analyzing systems at the end of the collimators. Unfortunately, this experiment failed due to blast damage, and was never attempted again in that form. Malik produced a comparatively simple device that allowed observation of the gamma-ray intensity above the ground surface and recorded the data underground, all of this being in a container perhaps one foot in diameter and several feet long. These devices could then be placed at several distances from detonations to observe the time history of a gamma-ray dose. It was the observations of the gamma-ray intensity with this device that allowed Malik to straighten out the initial arguments concerning the yield of the first large thermonuclear device (Mike). Both kinds of measurements were made on a great number of shots through practically all of the operations up to 1958. Parallel laboratory theoretical work combined with the field observations, including photographic evidence as to the position of the cloud and the time of cloud rise, etc., led to a fairly complete understanding of the initial processes and the transport phenomena, etc., that lead to a given dose at a given point in space from a nuclear detonation. Thus, by 1958, this subject was well in hand for normal atmospheric detonations. However, by then the reliability of the fireball technique for yield measurement and radiochemistry for both yield and other data was such that the measurement of gamma rays was no longer actively used to contribute to yield information.

Thermal Radiation

Outstanding observations of the thermal characteristics of the Trinity shot were made by Julian Mack and others. Very detailed, integrated and time resolved spectral observations were made, along with attempts at the total radiation flux, by various optical means.

Observations were made photographically with high-speed cameras on all operations. It was somewhat difficult to deduce from these observations the actual thermal fluences because of the very complicated calibrations needed for film sensitivity,. processing characteristics, optics, etc. These kinds of measurements were used to determine the absolute value of, and the time dependence of, fireball brightness, and in some of the later operations, appropriate filters were used to get some measure of the spectrum versus time.

The major effort after Trinity came when the NRL group under Wayne Hall took on the job, under Los Alamos auspices initially, to document this whole phenomenon. Preston Butler, of NRL, in conjunction with Group J-3 in Los Alamos, began to take spectrum measurements on Sandstone. Harold Stewart took over the job for Greenhouse. The need for measurements of the thermal radiation from nuclear detonations was recognized early on, since thermal radiation was one of the major effects to be expected in warfare as it was contemplated at that time. It was also expected that thermal output could be a good measurement of the yield, once understood. Since the thermal output as a function of time was directly connected to the initial stages of the blast phenomenon, or fireball expansion phenomenon, an understanding of the details of the thermal radiation was to assist in an understanding of fireball expansion, even though the thermal radiation from the shock front is a small portion of the total.

A massive program was therefore initiated for Greenhouse under Harold Stewart and Wayne Hall at NRL. That program included measurements of air attenuation from the bomb, that is, air attenuation over the path from the bomb to the receiver; very detailed high-resolution time integrated spectra; spectrum as a function of time taken on several instruments (both streak and framing cameras through spectrographs); thermopiles to attempt to measure the total thermal radiation; bolometers to measure radiant power as a function of time; and other instrumentation. The so-called blackball was invented. This was a simple device consisting of a hollow copper sphere approximately eight inches in diameter, painted black on the outside, with a maximum reading pressure gauge attached. The sphere was filled with gas (air). Thermal radiation impinging on the black surface heated the copper ball which gradually transferred its heat to the contained gas resulting in an increase in pressure. Therefore, a reading of the maximum pressure was directly related to total thermal radiation received from the bomb. These were very simple instruments that could be mounted at different distances from the detonations, were easily read, and, perhaps more importantly, collected the thermal radiation coming from all directions. The efforts of Stewart's group continued at high level through the whole period before the moratorium, sometimes under the direction of Lou Drummeter or Donald Hansen. Fantastic amounts of information were collected. Other experimenters, Sandia and various groups from the Department of Defense, entered this field of endeavor later on, but their efforts never compared seriously in the straightforward type of measurement with those of the group at NRL. Measurements were made on all the Pacific operations and all the Nevada operations. Hence, a great deal of information was collected on shots of various yields fired in various manners, but it is notable that no appreciable information was collected on high-yield, that is, megaton range, airbursts other than King shot. Coverage in the infrared was minimal.

On Upshot-Knothole (1953) and at later operations, these measurements were extended to include the so-called Chord experiments in which a fixed bright light placed some miles to one side of the detonation could be observed by highly resolving spectral instruments from another station, again placed several miles from the detonation, but in a manner such that the path of light passed fairly close to the detonation at a predetermined distance. The observation of the absorption bands, etc., could give information on those molecules formed in the air due to the gamma ray and neutron flux, or even x-ray flux, before the shock wave or fireball reached the light path. Enough analysis was performed on this great mass of data before the moratorium, mainly by NRL and the group under Herman Hoerlin of Los Alamos, to achieve a fairly complete understanding of the molecular processes taking place during the fireball expansion and of the absorption produced in air ahead of the fireball as well as other phenomena associated with the fireball expansion. These measurements showed, among other things, that the fraction of total yield coming out as a visible part of the spectrum did vary with yield, from about 45 percent at small yield to perhaps 25 percent for megaton shots. Eventually, calibration curves were devised and total thermal provided a moderately accurate measurement of yield, especially in the early operations, that is, the operations in the mid-1950s in Nevada. The measurements showed that the brightness of the fireball peaked at something like 10 to 20 kilotons, decreasing both ways from that to rather great extremes. For example, Ranger A was so cold that it showed line spectra from the components of the bomb. On the other hand, the very large bombs, 10 to 15 megatons, were sufficiently dim that they could *almost* be viewed with the naked eye safely. (However, for self-protection, no one was allowed to do that.) It is perhaps of interest to note that so much data were taken during those years that much of the spectral data still have not been analyzed, and important physical knowledge is still coming out of those data.

By the time of the moratorium, there were, counting Los Alamos and NRL, some 60 people working in the field on this subject in addition to the DOD and Sandia efforts. Through this long effort there came a great amount of theoretical and experimental knowledge which was used in developing the experimental plan for optical observations of Teak and Orange, the high-altitude shots of 1958, and even more in the theoretical predictions* as to the phenomena to be expected so that the instrumentation could be laid out properly. Thus, in 1957 and 1958, when the high-altitude shots of Hardtack were planned to gather information on the phenomenology of high-altitude detonations, a great amount of instrumentation and expertise was available, and Hansen and Hoerlin were of appreciable assistance in designing not only their own measurements on those shots, but those of other experimenters from other laboratories and from the Department of Defense. Unfortunately, both of those shots had operational difficulties so that very little of the close-in prompt data were obtained from Johnston Island. By this time, both the AEC Laboratories and the Department of Defense had learned to operate some of the optical gear in aircraft, and these were used on Hardtack. In spite of the lack of data, the experience of planning in detail for Hardtack and facing the operational problems gave the experimenters a great deal of experience which was of great value in the Dominic series.

Blast/Overpressure

The subject of blast is certainly on the borderline between outputs and effects measurements, but, since this point was under continual contention in the late 1950s, there is little reason to straighten it out now, and hence, it will be included here. Initial experiments to study the characteristics of the overpressure or blast as a function of distance from nuclear weapons were made at Trinity in 1945, specifically by Penney (later Sir William Penney and now Lord Penney) and others. Obviously, the basic rules of the propagation of sound through air had been studied for years before the advent of the nuclear weapon. However, not so much was known about the propagation of high-pressure shock waves through air and the theory of the mechanism of the formation of the shock wave in the stage of fireball growth was in very poor shape. Much depended upon the distribution of material throughout the fireball and upon the equation of state of the air in the shockfront of the fireball front as it was growing. The equation of state depends not only on the temperature, which was uncertain, but also on the specific states of the ionic, atomic, and molecular constituents of the gas, which varied with time due to exposure by x-rays, gamma rays, and neutrons, and by the varying recombination rates of many species. Even without the complication introduced by the uncertain atomic and molecular composition

*An appreciable proportion of the theoretical work was inspired by Hans Bethe, and carried out by Skumanich, Jahoda, and Stone.

of the "air," the interacting phenomena of radiation propagation and high-pressure shock propagation close to the time of breakaway were not well understood.

Some of the early instrumentation used at Trinity and Crossroads was remarkable for its simplicity and ingenuity, and even more remarkable for the consistency of the results produced. For example, Penney exposed sealed beer cans and five-gallon gas cans at several distances from the detonation in order to obtain a measure of the peak overpressure, the concept being that the can would crush to the point at which the internal pressure was equal to the external blast pressure. The cans could then be collected at leisure after the shot, and the volume change measured by simply pouring water in the can, pouring it out into a measuring device, and by very simple calculation deriving the overpressure. Unfortunately, this method had some difficulties. The cans did have some residual strength, requiring a correction at low overpressures; but there was some variation between cans in the crushing pressure required to get to a given volume. The temperature of the air inside could be changed by other phenomena than the shock wave and, hence, affect the volume to which it reduced for a given pressure. For example, the bomb's initial thermal radiation heated the can. The materials of the can did have some inertia, and, therefore, the volume finally achieved was dependent to a certain extent upon the temporal shape of the pressure wave. For instance, an initial very high peak would not be observed. Lastly, as was observed in later operations, the local surroundings of an observation point could produce anomalies that would affect the local overpressure. Because of the tremendous importance of an understanding of blast phenomenology and, in particular, the military need for tables which would give the overpressure as a function of distance and height of burst, etc., a great deal of effort was spent on this subject in the early years.

Greg Hartmann and his co-workers at the Naval Ordnance Laboratory (NOL, now called the Naval Surface Weapons Center) began to develop more detailed methods of observation and put them into effect during the Sandstone operation. Pressure gauges of various kinds were developed with appropriate time resolution to follow the major portion of the shock wave. It quickly became obvious that surface effects adjacent to the pressure-measuring gauge were important, so gauges were mounted on horizontal concrete surfaces or in radial walls. The formation of a permanent testing division at Los Alamos led, in conjunction with NOL, and through the auspices of Reines and Porzel and others, to a greatly expanded blast-measuring program. This led to a massive effort on Greenhouse in which new, improved surface gauges were placed both in ground surface installations and in specially constructed walls radial from the detonation. (The ground surface installations suffered greatly from the heavy rain at Eniwetok.) Efforts were made to take into account the particular characteristics of the air at the time of detonation, the wind direction, etc. In fact, small high-explosive detonations were used just prior to shot to get the sound velocity from the shot point to the detectors.*

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*One of the more exciting incidents of Greenhouse took place at a time when the arming party was in the tower preparing to arm the weapon. A member of the blast team was closing the last switches before evacuating the island and, due to a miswiring, managed to fire a five-pound high explosive on the tower not far below the cab. The arming party leader, Jack Clark, after recovering his equilibrium and allowing people to clean up the personal mess, set off in hot pursuit of the culprit and eventually found him in his little switch station at the other end of the island wondering why his circuits did not seem to be right. After the appropriate chewing out, the man closed the circuit again to show that everything was all right. The monitors immediately showed that it was not all right, and that was the end of the high-explosive part of the experiment on that shot.

At about this time, it was recognized that many other phenomena were affecting the shock wave measurements; in particular, the change in temperature of the air close to the ground due to the thermal burst, dust thrown up into the air from the initial thermal burst, especially in Nevada, etc. AFSWP began to take a larger and larger hand in the measurement of blast phenomenology, as did the Sandia Laboratory. Thus, during the mid-1950s a great spate of experiments were performed by various DOD contractors, Sandia, and Los Alamos to investigate these phenomena. Thermal measurements were made close-in to the tower shots and balloon shots at the NTS to establish the initial thermal pulse on the ground. Measurements of air density and dust loading were made close to the surface by various techniques including photography. Even the range of beta particles in the air as a function of time as the shock wave went by was measured in order to obtain the air density. The Department of Defense actually built a moderate-size lake at their Frenchman Flat site in order to compare the shock wave shape over land and over water for the same detonation. All of these measurements with the concomitant theoretical effort resulted in a fairly detailed understanding of shock wave formation and propagation and the effects of various surfaces on the shock wave shape. Unfortunately, essentially none of this work was performed on megaton bombs fired at altitudes pertinent to wartime use. However, the data were sufficient to establish height of burst curves for the military which, apparently, are still the ones in use. A great deal of the expertise on this subject was lost during the moratorium due to decreased budgets. Further measurements of blast and shock in air could not be made in Nevada on underground shots after the moratorium, and because the interest was on other subjects, very little effort was expended during Dominic on blast.

Electromagnetic Effects

As was noted previously under diagnostics, electromagnetic effects from nuclear detonations had been observed very early. It promptly became of interest, especially to the military (AFOAT-1, later AFTAC) and others, to document this phenomenon at comparatively long times. Both close-in and long-range measurements were made very early and continued on all of the operations through Hardtack. The interest in this subject stemmed from several concepts. Obviously, the electromagnetic signal might be used as an observational technique to detect a foreign detonation and it was possible, with sufficient unraveling, that the signal could give some diagnostic information about the detonation. With the advent of the planning for intercontinental ballistic missiles, especially the Minuteman with its silo complexes, there was worry that electromagnetic signals would be picked up by the interconnecting circuitry at the missile bases and in some way render the whole launch site ineffective at a very critical time (presumably under attack by a foreign detonation). Of course, there was also strong curiosity about the reasons for the formation and shape of this signal. Close cooperation was maintained during these years between the AEC experimenters (such as Malik, Wouters, Watt, and Partridge) on this subject and their corresponding Department of Defense colleagues, and appreciable contribution to the understanding was made by the British through the JOWOG* meetings on the subject.

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*JOWOG--for Joint Working Group, which was established to implement the terms of the 1958 agreement between the government of the United Kingdom of Great Britain and Northern Ireland and the government of the United States of America for cooperation on the uses of atomic energy for mutual defense purposes.

Experiments were performed to measure the field strength as a function of distance from the bomb. An east-west effect was noted on the polarity of certain portions of the signal. The observation of the characteristics of the signal for different types of detonations, that is, airdrop, surface, or tower, and for different types of devices, small yield or large yield, boosted, etc., led to a gradual unraveling of the reasons behind such a signal, a great portion of the work being done by Suydam, Malik, and Wouters. Nevertheless, by the time of the moratorium in 1958, there were still gaps in the understanding of this phenomenon and, unfortunately, just at that time, because of the installation of Minuteman sites, an understanding was becoming more and more important. The AEC Laboratories could and did offer "rule of thumb" precautions to take against upsets of the Minuteman system, but it took the construction of simulators and field experiments during the moratorium to eventually lead to some satisfaction that the sites were safe. Obviously, there is still some uncertainty on this problem.

Various other phenomena were investigated during this period that will not be gone into in detail here. Observations of the ionospheric changes due to high-yield detonations were made by the Department of Defense and contributed to one of the later systems for the detection of foreign nuclear detonations. Observations of the changes in the earth's electric and magnetic fields at moderate distances were made in the Nevada shots, and Fred Reines even considered the use of a nuclear detonation as a source for the observation of neutrinos but eventually decided a reactor was more sensible.

Effects Experiments

During this period of time, a great number of experiments were conducted by the Department of Defense to determine the effects of weapons outputs on materiel and people. The initial experiments were conducted by the separate Armed Forces and later on by the Armed Forces Special Weapons Project formed on January 1, 1947. The growth of the Civil Defense effort in this country, beginning in 1954 and 1955, led to another set of such experiments emphasizing civilian protection considerations. Some of these were conducted by various health organizations of the AEC Laboratories or AEC Headquarters. Large efforts were expended at Crossroads and Sandstone on military effects. Between 1950 and 1959, some 1,700 separate reports were written on the results of effects experiments conducted in conjunction with nuclear tests. Those reports were written by authors from over 100 experimental organizations, mostly under Department of Defense cognizance. Only a brief overview of the subject can be given here.

The Hiroshima and Nagasaki detonations, while clearly not experiments but the only wartime use that has ever been made of nuclear weapons, furnished in the few years after 1945 a great deal of information on the effects of nuclear weapons, especially on people. The United States at that time occupied Japan and, hence, could carry out postshot investigations with great thoroughness. Unfortunately, while the yield of the Nagasaki "Fat Man" Christy device, the same design as used in Trinity and Crossroads, was fairly well known, the yield of the Hiroshima "Little Boy" device was never determined with sufficient accuracy for evaluation of the Japanese effects data. Many attempts were made in later years to reconstruct the Hiroshima experience, even including the serious suggestion that the device be built again and fired in Nevada. But by then certain detailed documentation necessary to reproduce the device had been lost, if it ever existed. Sir William Penney tried to determine its yield by observing the blast effects on various containers found in the streets of Hiroshima but could never get consistent results. Postshot observations of apparent thermal flux and neutron flux were also used but all proved too inaccurate. Nevertheless, a great deal of information was obtained on the effects of thermal burn, of high-level radiation doses, and of the blast effects on Japanese structures, some of which were of similar construction to American structures.

At Trinity very few true effects measurements in the sense of this section were made. Bill Penney did observe the effect of radiant heating in igniting structural materials. It was intended that B-29 aircraft would be in such a position as to experience effects similar to those that might be expected in the upcoming drops over Japan, but rainy weather delayed the shot, and hence the aircraft were not properly positioned.

As mentioned before, the first postwar operation was solely for effects purposes, and used the then stockpiled MK3A Christy device as the source. Crossroads was set up by the United States Navy to investigate the effects on ships of a nuclear detonation. The Navy was particularly concerned with the problem of a detonation in a harbor and, hence, sought out a lagoon, ending up at Bikini in the Marshall Islands. The Navy had a number of outmoded U.S. military vessels that could be used for this experiment, rather than being scrapped, and also had a few captured Japanese and German vessels.

Two experiments were performed. The first was to determine the effects on ships of an airburst over water, and the second was to look at the effects of an underwater detonation. The airdrop was fired first (20 kt at 520 feet) because it was expected to do less damage than the underwater shot. Hence, it would leave ships for experiments on the later shot. The airdrop, while producing serious effects, did not do quite the damage that had been expected. But the second shot (20 kt at 90 feet depth) was spectacular. Whole ships rose up in the water spout produced, and many of the ships immediately went to Davy Jones' Locker. The radioactive contamination on the ships remaining was sufficiently startling as to color the Navy's thinking on that subject ever after.

The Navy learned a great deal about the effects of airblast and underwater shock on ships as a result of these two detonations. In general, ships suffered serious damage or were sunk at air overpressures greater than 10-12 pounds per square inch, and were damaged above 4 psi. Boilers and deck structures seemed especially vulnerable. Lethal water shock overpressure was in the 3,000- to 4,000-psi range.

Crossroads was also the beginning of the DOD effects efforts in a number of other fields. Biological experiments were conducted using sheep, dogs, etc.* Blast and thermal documentation were carried out. Water waves were measured. Effects on the ionosphere were noted. Radiological observations were made, etc.

During those years, in addition to conducting experiments on AEC-sponsored shots, the Department of Defense sponsored a number of detonations solely for effects measurement purposes. A partial list follows in Table IV.

The effects efforts during the late 1940s and early 1950s were guided by the need to understand the effects of nuclear detonations fired as then militarily deliverable, that is, airbursts, cratering bursts, underwater bursts, and surface bursts. As missile delivery became more feasible, attention turned to the effects of high-altitude and deep space detonations.

The earlier work was devoted to understanding and learning to predict the weapon outputs, and the effects of those outputs on things and people. So the effects community supplemented AEC device output measurements of neutrons, gamma rays,

*Operationally, it was most interesting to note the placement of these live animals before the shot and somewhat hilarious after the shot, because great numbers of the animals were swimming around the lagoon being chased by their owners.

TABLE IV
DOD-SPONSORED EFFECTS SHOTS
(1946-1958)

Operation	Shot	Date	Purpose	
Crossroads	Able	06/30/46	Airblast on ships	
	Baker	07/24/46	Water shock on ships	
Greenhouse	Easy[a]	04/20/51	Structures, blast	
Buster-Jangle	Jangle S	11/19/51	Effects of small-yield	
	Jangle U	11/29/51	Surface and cratering detonations	
Tumbler-Snapper	TS-1	04/01/52	Terrain Effects	
	TS-2	04/15/52	Terrain Effects	
	TS-3[a]	04/22/52	Terrain Effects	
Upshot-Knothole	Encore	05/08/53	Terrain Effects	
Teapot	ESS[a]	03/23/55	Underground effects	
	HA	04/06/55	High-altitude (36,620') outputs	
Wigwam	Wigwam	05/14/55	Radioactive/underwater shock phenomena	
Plumbbob	Priscilla	06/24/57	Vulnerability and Effects shot; [redacted]	5 U.S.C. 552(b)(3)
Hardtack I	Yucca	04/28/58	High-altitude (86,000') effects	Ex. 3, D.O.E.
	Wahoo	05/16/58	Underwater effects (500')	
	Umbrella	06/08/58	Underwater effects (150')	
	Teak	08/01/58	High-altitude (252,000') effects	
	Orange	08/12/58	High-altitude (141,000') effects	5 U.S.C. 552(
Argus	3 shots	08/27-09/06/58	Deep space [redacted] effects	Ex. 1, D.O.E.

[a]Cosponsors with AEC.

thermal radiation, and blast, gradually taking over some of the measurements completely. At the same time, they investigated the effects of these outputs on airplanes, tanks, jeeps, clothing, docks, housing, underground shelters, animals, ships, etc.* They studied the effects on radio and radar propagation, that is on the ionosphere. Long-range detection schemes based on these phenomena were put into operation. Methods of predicting and detecting radioactive fallout were investigated.

*Perhaps one of the most outstanding effects measurements in Nevada from the point of view of the outsider was the experiment intended to be an observation of the effects of the blast wave from nuclear detonation on blimps. Several operating blimps were brought to Nevada, and appropriate mooring towers established for them at the proper distance from the expected detonation. It was important that the wind be blowing in the right direction since it was intended that the blimps be head-on to the shock wave. After a number of operational difficulties in which one blimp got loose for a while, the experiment was performed. The expectation was that since the surface of the blimp was fairly flexible, the shock wave would pass through the gas inside the blimp just canceling the shock wave pressure outside, and that no particular damage was to be expected. However, as anyone could have told them, but no one did, the velocity of a shock wave is different in helium than it is in air. Specifically, it is faster. Therefore, the shock entered the front end of the blimp as expected, but by the time it had reached the rear end, the shock wave inside the blimp was appreciably ahead of the shock wave outside. So the entire pressure differential was exerted against the rear end of the blimp and blew it right out, with the concomitant effects on the airworthiness of the machine.

~~SECRET~~

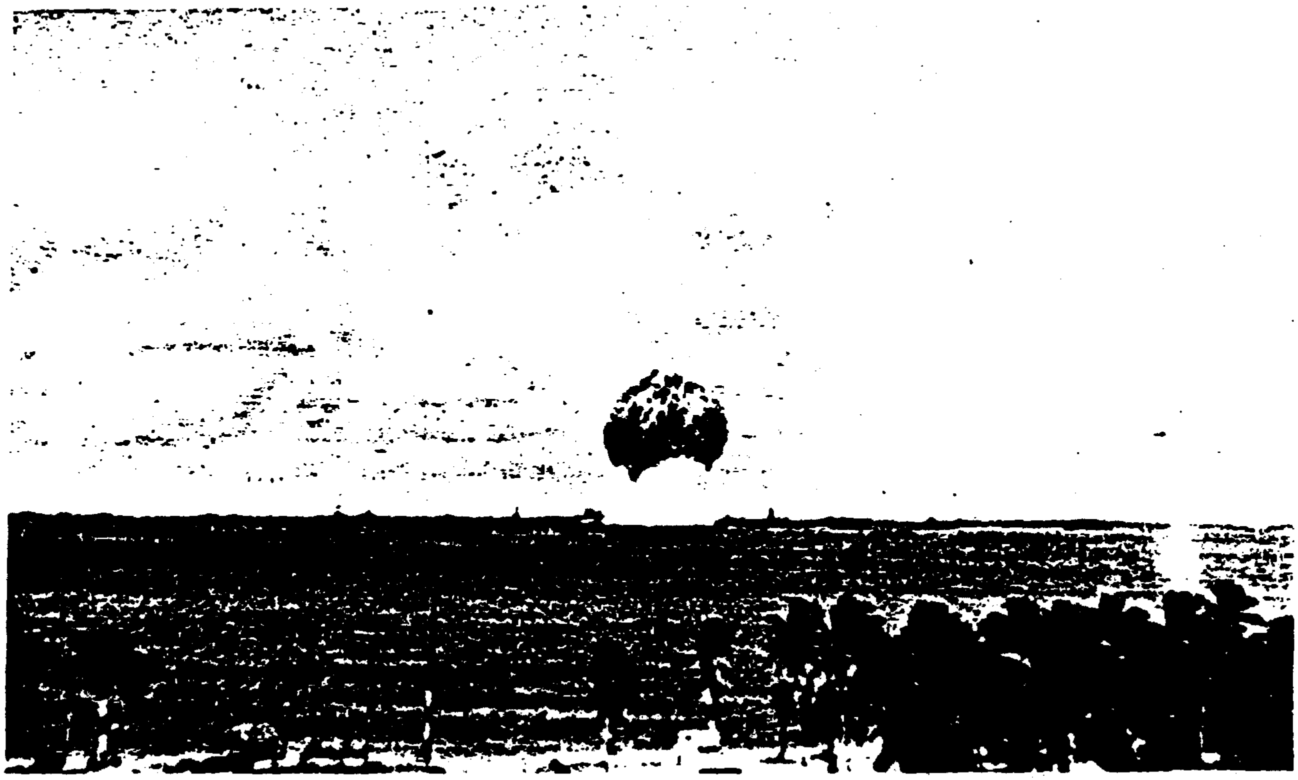

Figure 9.
Crossroads Baker just emerging; note ships.

In the late 1950s, because of growing concern with the intercontinental ballistic missile and antiballistic missile systems, appreciable attention was turned to high-altitude detonation effects. Blast and thermal phenomenology were expected to be strongly different than for sea-level detonations. X-rays would become important. Bomb debris itself could get into space, perhaps showing effects in other parts of the world. Radio and radar propagation could be seriously affected. As early as the Fizeau shot (9-14-57), Sandia was investigating the effects of the fireball on [redacted]

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During this time (1946-1958), the efforts to understand the effects of bomb outputs on people (and other animals) were also widespread. Dogs and other animals were exposed to air shock to determine the damage mechanisms. The detailed mechanism of neutron and gamma interactions with cells were studied. Skinburn and eyeburn criteria were determined. The effects of radioactive material on the skin or after ingestion were documented. Of particular note is the work of Lauren Donaldson and his co-workers at the University of Washington who have documented animal and man radiation effects at the Eniwetok Proving Ground from 1946 to the present.

As the result of the massive effort on the part of the effects community, by the time of the moratorium, the effects of low-level or surface nuclear bursts were in general adequately understood. Cratering for small shots at about "optimum" depth had been documented (although the effects for very shallow bursts were still hazy). However, the effects of high-altitude detonations were still very uncertain. On some subjects, the knowledge was still too dim to ask even the right questions.

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Systems Tests and Operational Exercises

The Hiroshima and Nagasaki airdrops of August 5 and 9, 1945, were, of course, the first nuclear weapons systems tests even though performed in wartime. As all the world knows, they were successful in that the mission was completed, the bombers were able to get away from the nuclear detonation safely, and the devices operated properly.

Thus, Crossroads Able in 1946 was the third test of the airdrop capability and did show up a difficulty. The bomb missed the target by some 700 yards. The normal explanation is that it "planed" immediately after leaving the aircraft and, hence, followed the wrong trajectory.* In the period between 1946 and 1958, a great number of devices were delivered by military aircraft. The bombs of the Ranger operation in early 1951 were airdropped from a B-50. A large fraction of the Buster, Tumbler-Snapper, Teapot, and Upshot-Knothole operations were airdrops. The 500-kt King shot of Operation Ivy in 1952 was dropped from a B-36H aircraft, and the [redacted] Cherokee detonation of Operation Redwing in 1956 was dropped from a B-52B aircraft. In the strictest sense, none of these were systems tests in that the devices were, in general, not yet stockpiled in their operational configuration, but in many cases, the shapes dropped and their weights and aerodynamics were identical to stockpiled devices and only minor modifications were made in the bombing aircraft, usually simply to arrange a radio link to start timers at the moment of bomb release. No serious genuine system difficulties were noticed during this period of time, although many minor things were observed and corrected. There were, of course, normal mechanical aircraft difficulties.** Human error was occasionally experienced.*** At the request of the technical side of the house visual bombing was used almost completely. However, there was radar backup.

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On July 19, 1957, the Air Force conducted a test of [redacted] air-to-air missile at the Nevada Test Site. [redacted] The missile was fired [redacted] and detonated at 20,000 feet. The crew [redacted] received 4 R, but there was no observable dose to observers on the ground.

5 U.S.C (b)(Ex. 3, D.O.

Thus, by the time of the moratorium, the Air Force had had a large number of experiences that were essentially systems tests using small bombs in Nevada, had gone through two airdrops in the megaton range in the Pacific, and had conducted one air-to-air missile test.

While the Navy conducted during this period of time a number of effects tests, the most notable being Crossroads in 1946, no genuine Navy systems tests were conducted.

The Army conducted its first and only true systems test in Nevada at Operation Upshot-Knothole. The Grable test of May 25, 1953, was the test of a Mark 9 artillery shell fired from a 280-mm gun. The only notable operational change between the manner in which this shot was conducted and the manner it would presumably be used in the field came about because the scientific advisor at that time, Al Graves, was not convinced that there was no possibility of the shell going off in the gun barrel. The Army, therefore, arranged the simple mechanism of a cable from the triggering

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*Listening on board ship at the time of the drop, the author remembers that the bombardier commented immediately that he had "tossed that one," possibly implying some error on his part.

**For example, at Ranger, Hoyt Vandenberg, who was at the Control Point for one of the shots, noted "The Air Force doesn't seem to be able to get rid of its built-in oil leaks."

***The "pickle barrel" in Nevada was occasionally as large as 1,600 feet in radius. The Cherokee airdrop of Operation Redwing missed by approximately five miles due to human error.

mechanism of the gun over a pulley attached to a lead brick. The lead brick was held on a small platform by a dogging mechanism which was actuated by a DN11 relay from the timing system. This simple replacement for a man operated satisfactorily, and the shell detonated at the proper altitude in a satisfactory manner.

The Army conducted a number of nuclear troop-training exercises in the mid-1950s in Nevada. The point was simply to acquaint some portion of the Army's forces with the circumstances surrounding a nuclear detonation. In general, the troops were brought into the region of the test detonation by truck and marched to prepared trenches or foxhole positions which had been placed in positions agreed upon between the Army Commanders and the Test Director. The troop positions had been determined by the Test Director to be safe from the point of view of blast, thermal, neutron, and gamma radiation. The troops, in general, crouched in the trenches while the devices went off, and were allowed to look up after several seconds to see the detonation. After experiencing the blast wave, they were again marched out. Through those exercises a representative cadre of Army personnel learned that Army maneuvers could be performed, within limits, on a nuclear battlefield.

Teak and Orange shots of Operation Hardtack in 1958 had many of the aspects of an Army operational systems test. The warhead carrier, a Redstone missile, was an early Army delivery system. However, the warhead was different than the operational system, and the guidance system had to be altered slightly to take care of the safety considerations demanded in that peacetime detonation. As mentioned elsewhere, the change led to the Teak and Orange shots going off at the wrong position in space.

No Marine systems tests were conducted during this period.

In retrospect, probably the most beneficial training to the Armed Forces, in a sense, came about from the policy of placing many military people in the AEC Laboratories as staff members, both to help conduct the operations and to work in other related weapons fields. The people generally stayed for two or three-year tours and were integrated intimately into the laboratory work, both at Los Alamos and Livermore.

Summary of Measurements

In general, the period 1945 to 1958 saw the development of a vast array of weapon diagnostic techniques, many of which could be altered to be useful on underground shots. The period saw the collection, compilation, and theoretical understanding of the effects of nuclear weapons fired low in the atmosphere, on the ground surface, or underwater, and saw a great growth of knowledge in the military on the possible uses of nuclear weapons in "conventional" warfare. However, knowledge of the effects of detonations at high altitudes was still very primitive.

Organization

The field organizations varied appreciably over the years 1945 to 1958. To a certain extent, the organizational structure, especially in the upper levels, was dictated by the responsible Washington-level agencies. Trinity in 1945 was somewhat unique in that the major technical organization (Los Alamos Project Y) was a part of the branch of the armed forces (Army) responsible for the whole nuclear weapon effort, and hence the effort was all "in house." By the time of Crossroads (1946), the Atomic Energy Commission had been formed, so the problem of proper assumption of authority and responsibility between federal agencies reared its ugly head, never to be really settled to everyone's satisfaction during the period of interest. The problem was not particularly serious on Crossroads, or on the similar later operation Wigwam, because the tests were clearly for effects purposes under the military, and AEC help was required more as a service, although it was never completely one-sided. In the later Pacific operations, 1948 through 1958, where the major purpose was clearly AEC, but the management was military, serious management problems arose. Those management problems never seemed to affect the actual conduct of the operation in any measurable fashion, but were usually serious enough to result in recommendations for organizational changes at the end of each operation. At Trinity, the overall administrative head, K. T. Bainbridge (he seems not to have had a more descriptive title), was part of an organization under direct contract to the Army, and the line of authority to him from General Groves and Oppenheimer was apparently clear and simple. However, when Sandstone (1948) was being put together, the Test Director (Darol Froman) was appointed, and then the AEC, feeling that the large amount of military support needed should not be under the command of a civilian (and knowing that the military would probably not agree to such an arrangement anyway), requested that the military supply a Task Force Commander. In a short time, the Test Director found himself three lines down in the organization chart, without the real authority to guide the operation in the manner he thought best. Fortunately, in that operation and in the later Pacific operations, the personalities involved were such that serious conflict was normally avoided.

The Task Force Commander for Pacific operations in general reported to the Joint Chiefs of Staff, through the particular Chief representing his service. During operational periods, he was also designated the senior representative of the AEC by the Commission, in order to have the top responsibility in the field embodied in one man. However, the Commission also usually made it clear to the Scientific Deputy Commander that he was expected to guard their interests.

After Sandstone (1948), the AEC and the military agreed that the man in charge of the technical work of the operation would be at a level just below the Task Force Commander, and would be designated "Scientific Deputy Commander." In order to assist the Commander, the military also designated military deputy commanders. Early on, the commanders of the Task Groups, the next operational level down from the commander, usually outranked the military deputies. That situation was reversed in later operations, in order to give the military deputies a more responsible role. Neither situation was really satisfactory in the period from 1948 to 1958 because the work of support by any one service did not require the efforts of two senior men. (However, this redundancy became valuable in 1962.)

There was formal agreement that all of the technical projects to be conducted in a Pacific operation would be under one man from the beginning. The intent to make the Technical Director second in command was always difficult to arrange formally. In practice, except for momentary flurries, it always worked that way because of the personalities of the personnel involved. Since that one man was (from 1948 to 1958) from the AEC side of the house, two levels of difficulty continually arose. At the Deputy Commander level (Task Force), the military deputies, and sometimes the services they represented, tended to resent, or dispute, the apparent seniority of the scientific deputy, and occasionally the Task Force Commander got tangled up in the problem. The upgrading of the rank of the military deputies after Greenhouse exacerbated the problem somewhat. Within the technical community, the AFSWP (Armed Forces Special Weapon Project) doubted the impartiality of the Scientific Deputy, especially in the later operations, and arranged for a military deputy to the Scientific Deputy. That deputy was always helpful. In a similar vein, when Livermore began to test nuclear devices, they too asked for a deputy.

Beginning with Sandstone, the work of the Task Force was divided among "Task Groups." One of these contained all of the experimental programs and projects. The others were thought of as support groups, although on occasion some technical project was assigned to one of the support groups for various reasons (for example, the work of AFTAC). Initially, the senior technical man was the head of the technical Task Group; however, when he was moved to higher level, another senior man was picked to run the Task Group. The relationship between these two men was also initially hard to define. Again, the situation could have become difficult if it had not been for the personalities of the individuals concerned. In practice, it seemed that the main job of the Scientific Deputy (or Scientific Director), aside from his safety responsibilities, was to assist the Technical Task Group Commander in his negotiations with the rest of the Task Force in order that he could accomplish his job.

On occasion, there was a problem brought about by the Task Force Commander getting involved with the DOD experiments in such a manner as to give them a different aim than that intended by the sponsors. Sometimes this helped, sometimes it did not.

Between Sandstone and Greenhouse, a permanent test division was set up at Los Alamos (J-Division). That division not only had the responsibility to plan and carry out the nuclear test work of the laboratory, but by agreement with the AEC Headquarters and the Department of Defense also carried out the administration and planning for the other technical agencies. Thus, through the auspices of "Task Group Point One," a single agency coordinated the technical planning between overseas operations and acted as the administrative agency for that work during the operations. In order to assist, the DOD assigned people to that group in Los Alamos, sometimes amounting to 70-90 people. Later on, representation was also furnished by other users, such as the Livermore Radiation Laboratory. This group dealt directly with the experimenters in arranging such things as physical layout, shipping, communications, construction, classification, etc. It acted as the administrative link between the experimenters and the outside action agencies, such as the Task Force headquarters and the other Task Groups. The existence of this permanent planning group established continuity between the overseas operations after Greenhouse. The group also assisted appreciably in Nevada operations, but only within the framework of the permanent Nevada Test organization.

Looking back, probably the major difficulties in the Pacific operations arose because of a basic inconsistency in aim. There was usually an urgency to start the operation on time and finish it as soon as possible (sometimes Presidentially directed). This urgency could be produced by programmatic aims, economics, or political consideration, or simply the desire to get the operation over with and go home. (A common statement was, "This delay is costing us a million dollars a day.") The personnel of the administrative structure usually felt this urgency strongly. On the other hand, each shot was being fired for a purpose, and each experiment was being performed for a purpose. Most important, the line of responsibility for the success of those shots or experiments was not through the temporary Task Force structure, but through the permanent AEC Laboratory or AFSWP structure. Thus, a person on the technical side of the house might sometimes feel that the shot was being fired without purpose because he was not properly ready to make the appropriate measurements, whereas the person in the administrative line might feel that the need to get the operation over, to get the right weather, etc., should override the needs of a particular experimenter, especially if it were a comparatively small experiment. This tug-of-war eventually led to agreed-upon lists of experiments that had to be ready before the shot could be fired, lists of other experiments that had to take their chances. A great deal of effort at higher staff levels was expended in continually trying to balance the conflicting points of view, and it is to the credit of all of the administrative people, on both sides of the house, that the operations were eventually conducted within moderate time limits, for reasonable cost, fairly safely, and with a high return of experimental data. Toward the end of the period, the suggestion of continuous testing, at a lower rate, was made by a number of organizations. Such a system may well have reduced the philosophical conflict noted above.

In Nevada, the situation was different. While appreciable military support was needed, the major "housekeeping" functions of transportation, housing and feeding, shipping, security, etc., could be done by the civilian side, so it was agreed at the Commission and Military Liaison Committee level early-on that the Nevada Test Site would be operated by the Atomic Energy Commission. The AEC appointed a "Test Manager," initially out of the Albuquerque Operations Office, and later from the Nevada Operations Office, to be responsible for test operations. The test manager had no responsibility or authority with respect to the technical program. Operations at NTS allowed an organization much more consistent with the internal Laboratory or AFWSP structure, with military support being integrated, but not controlling. By agreement, the Test Manager appointed a "Scientific Director" or "Scientific Advisor," initially from Los Alamos. Later on, the appointment to the position alternated depending on the sponsor of the particular shot. In the early operations in Nevada, a single "Test Director" was responsible for all experimental projects, but the growing test program of the Livermore Laboratory eventually made that system unsatisfactory, so that "Test Groups" were formed, allowing each major test organization to have its own "Test Group Director," responsible directly to the Manager (and the sponsoring organization). At approximately the same time, the area of the test site was divided in such a manner as to reduce interference between the users.

In general, the Nevada operations seemed to go somewhat more smoothly than those in the Pacific, partly because they were smaller and simpler, and because the participants were closer to home and hence did not feel so captive, but mostly because the chain of command was only slightly skewed from normal by the test command structure.

Other

A number of other competencies needed for nuclear weapon testing were developed during these years, but will not be covered in any detail here. Most important perhaps was the development of the radiation safety (rad safe) structure in both the AEC and DOD. Measurement and prediction ability grew as a result of the large efforts put in on both sides of the house. This work went hand in glove with the continued effort to understand the effects of radiation. The prediction capability depended strongly on input from the weather prediction units, also gradually developed to work with the rad safe prediction units.

Field construction was handled with growing competence during this period by several companies, the most outstanding being Holmes & Narver (H&N) in the Pacific, and Reynolds Electric and Engineering Company (REECo) in Nevada. Their expertise was essential to the return to testing in 1961-1962.

Other functions, such as shipping, the care of legal problems, security, and safety, were handled by people of growing experience in the nuclear weapon test field.

Of great importance, a small group of people with great and broad competence in the various nuclear weapon effects, and with understanding of operational problems, had come into existence. Sometimes associated with "weather panels," "safety panels," "advisory panels," or with more specific problems, they furnished a cadre of trusted judges to whom the Task Force Commander, a Scientific Deputy, Test Manager, or Scientific Advisor could turn for guidance when the chips were down. In a number of operations, there was a tendency to leave this group off the organization charts, but their help was of great value. In no particular order, some of these people were A. Vay Shelton, O. W. Stopinski, L. Joe Deal, Carter Broyles, Ralph LaChavese, Gordon Dunning, Clint Maupin, Mel Merritt, and John Malik.

Prologue Summary

During the period of 1945 to 1958, the British, Americans, and Russians tried, both through the auspices of the United Nations and by separate conferences, to arrive at an appropriate agreement for arms control and specifically for the control of nuclear weapon testing and stockpiling. These attempts were in general not successful, in part because of the Russian need to establish a nuclear weapon capability of their own and in part because of the United States insistence on "adequate" control systems. In the late 1950s, because of the rapid growth of Russian nuclear weapon capability, and because of worldwide reaction to the "dangers" of radioactive fallout, the pressure to halt nuclear weapon testing grew strong, and by late 1957, Eisenhower was feeling that pressure and seeking ways to come to some agreement on the subject.

Advancement in American nuclear weapon design was great. [redacted]

5 U.S.C. § (b)(3) Ex. 3, D.O.E

[redacted] out during the period. Towers, barges, balloons, airdrops, underwater, underground, and rockets were all terms that became familiar. However, by the end of 1958, balloons in Nevada and barges on the Pacific were the most commonly used platforms for testing.

Permanent proving grounds had been established in the Pacific and in Nevada, with permament on-site staffs. The major testing organizations all had permanent testing groups. By the end of 1958, a seasoned, experienced, testing organization existed and was operating. But by the end of Hardtack Phase II, it was tired.

Diagnostic methods were developed during the period beyond that available at Trinity. The reaction history could be measured in great detail. The observation of radiation flow and thermonuclear burn was well advanced. The gamma ray, neutron, thermal, blast, and electromagnetic outputs of nuclear devices over a wide range of yields had been measured for sea-level detonations, and were moderately well understood theoretically.

The effects of sea-level detonations were investigated in great detail. Blast and thermal effects on ships, buildings, animals, etc., were tabulated. Both prompt and delayed radiation effects were well understood by 1958. Fallout predictions and the predictions of other hazards could be made with sufficient accuracy for operational decisions.

[redacted]

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In short, by 1958, there was a mature nuclear weapon design and testing system, nuclear effects from sea-level detonations were well understood, the world was afraid of atmospheric nuclear weapon tests, and we were just beginning to learn how to test underground. Many of us did not want to learn, ever!

CHAPTER I

PREMORATORIUM INTERNAL READINESS ACTIVITIES

The AEC, the weapons laboratories, AFSWP, and the other components of the nuclear weapons "complex" had been intimately involved with the national actions toward a nuclear weapons test ban since the subject was first raised. The early Lillienthal group had several members from that complex. The complex was continually consulted on the subject through the early years, and an appreciable amount of "in-house" effort went to those considerations.

After the exodus from Los Alamos at the end of WW II (and from other portions of the system), the new weapons complex consisted of people who probably, on the average, felt that the continued design, testing, and production of nuclear weapons was a necessary element in the United States defense posture. When a test ban, as a prelude to other disarmament moves, began to be considered seriously, the nuclear weapons complex spent some effort considering the effects of such a ban on the weapons system, what capabilities should be maintained, and what moves, ahead of time, might "safeguard" the system capabilities.

While there were some early opinions expressed that in order for a CTB (Complete Test Ban)* to be believable to the rest of the world, the weapons complex would have to be completely dismantled, including the dissolution of the weapons laboratories, these opinions were never taken very seriously. Rather, the feeling seemed to be that any such agreement should be entered into gingerly, that the Russians were not trustworthy, and that therefore, the weapons complex should be maintained, at least for a few years. The stockpile needed "care and feeding," and further advances in nuclear weapon system design could clearly be made, and might become necessary were the Russians to act in some inappropriate manner.

However, the aims of "maintaining the weapons complex capability" or "maintaining a nuclear test capability" did not, in general, lead to clear-cut and generally agreed upon suggested actions. There was clear agreement that the internal health of the AEC weapons laboratories had to be maintained (the point was not so clear with respect to the DOD laboratories), but what did that mean? Should the people be kept at work on weapons design and production problems, or should their capabilities be exercised by putting them to work on other subjects? Without nuclear testing, would it be possible to keep them on weapons work very long? Would good people stay to work on problems that could not come to fruition? To maintain a testing capability, was it necessary to maintain the proving grounds? Were cadres representing the major field contractors (EG&G, H&N, REECo, etc.) necessary, or could these organizations be allowed to disappear? Was it necessary that the in-house weapons test organizations be maintained as entities, or could they be absorbed into the other parts of the laboratories? If they were maintained, what work should they do?

These questions were not taken particularly seriously over the years 1946 to 1956, but began to use up more effort as the moratorium approached. The separate

*Today, July 1979, CTB means Comprehensive Test Ban.

organizations, as was to be expected, took somewhat different tacks in answering the questions. However, once in a while there was consistency. In June 1954, Teller* (Livermore) and Bradbury** (LASL) sent their joint opinions to Ken Fields (General Manager, AEC) in response to a query by John Foster Dulles. They felt that a CTB would work in favor of the Russians because the United States would observe the treaty -- but it might be circumvented or openly violated by the Russians -- and because the Russian intelligence (due to the difference in societies)*** was better than that of the U.S., so the only way the U.S. could stay ahead was to work harder and faster, which it could not do under a CTB. They further commented that a TTB (Threshold Test Ban) would have the following effects:

a. If the threshold were zero, there would be no tests, and hence little or no progress, accompanied by a loss of sense of urgency.

b. If the threshold were 5-10 kilotons, the U.S. could do tactical weapon development. They recommended that at least this be allowed for any condition short of complete and satisfactory atomic weapon control.

c. If the threshold were 50-100 kilotons, they could do weapon component testing for large bombs. They recommended that the threshold be at least this high.

d. If the threshold were 1-2 megatons, they could develop lightweight thermonuclear warheads. They opined that such a threshold might impede the Russians, who seemed more interested in very large yields.

e. If there were no limit, the laboratories would increase their capabilities in the high megaton field.

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*Edward (Ede) Teller--born January 15, 1908, Budapest, Hungary--Inst. of Tech., Karlsruhe, Germany, 1926-1928--Ph.D., U. of Leipzig, Germany, 1930--numerous D.Sc.s, etc.--Rockefeller fellow, Copenhagen, 1934--Lecturer, U. of London, 1935--immigrated to U.S., 1935--Prof. Physics, George Washington U., 1935-1941--naturalized, 1941--Columbia U., 1941-1942--U. of Chicago, 1942-1943--Site Y (Los Alamos Laboratory), 1943-1946--U. of Chicago, 1946-1949--Los Alamos Scientific Laboratory, 1949-1951--U. of Chicago, 1951-1952--Livermore Laboratory, 1952-1975--Retired, June 1975. Participated in the early 1939 American work (Szilard, Tuve, Rosenfeld, Wheeler, Hafstad, Zinn, Fermi, Anderson, etc.) showing the possibility of a uranium 235 bomb--with Szilard visited Einstein Aug. 2, 1939, to obtain his signature on the letter to Roosevelt that led to the establishment of the "Advisory Committee on Uranium" (Oct. 1939)--consultant to that committee 1939-1941--with Fermi (fall 1941) calculated the feasibility of a thermonuclear bomb--assisted in production of world's first nuclear chain reaction, Stagg Field, 1942--presented the thermonuclear concept to the June 1942 Berkeley conference on atomic weapon progress--Group Leader, T-1, Los Alamos, 1944, hydrodynamics of implosion, Super--member, Los Alamos Tech. Board, July 1944--Group Leader, F-1, the Super and General Theory, Sept. 1944--Group Leader, T-7, Super, Nov. 1945--Observer, Trinity, July 1945--Asst. Director, Los Alamos Scientific Lab., 1949-1951--led conceptual work that invented "secret" of the thermonuclear bomb 1951--pressed for second nuclear weapons laboratory 1952--joined Livermore 1953--Assoc. Director 1954-1958--Director 1958-1960--opposed complete test ban and proposed underground testing as alternative 1957-1962--pressed for "clean" weapons. Member, USAF Scientific Advisory Board--fellow, American Nuclear Society--fellow, American Physical Society--member, National Academy of Science--others--Albert Einstein Award 1958--Fermi Award 1962--others.

**Norris Edwin Bradbury--born Santa Barbara, Calif., 1909--Whiting Fellow 1931-32--Ph.D. (Physics), U. of Calif., 1932--NRC fellow in physics, Mass. Inst. Tech., 1932 to 1934--Asst. Prof. Physics, Stanford U., 1934-1937--Assoc. Prof. 1937 to 1942--Prof. 1942 to 1950--Prof. Physics, U. of Cal., 1950--active service, U.S. Naval Reserve (Commander), 1941 to 1945--Dahlgren Naval Proving Ground (exterior ballistics) 1941 to 1944--joined Site Y (later Los Alamos Laboratory) July 1944--Interdivisional Weapons Committee (responsible for all phases of nuclear weapon work peculiar to combat delivery) 1944--Group Leader X-1 (implosion research) Sept. 1944--Group Leader X-6 (weapon assembly), Mar. 1945--Technical Deputy, Project Alberta (activities concerned with combat atomic weapon delivery), Mar. 1945--Group Leader, TR assembly Project TR (Trinity), June 1945--Director, Los Alamos Laboratory, Oct. 1945--member, USAF Scientific Advisory Board--member, Science Advisory Committee, Office of Defense Mobilization, 1955-1957--retired, Sept. 1970. D.Sc., honorary, Pomona--D.Sc., honorary, Case--LL.D., honorary, U. of N.M.--fellow, American Physical Society--fellow, National Academy of Sciences--Phi Beta Kappa--Sigma Xi--Navy Legion of Merit 1945--Special Certificate, U. of Cal. Regents, 1960--DOD Distinguished Public Service Medal 1966--AEC Citation 1968--Fermi Award 1970.

***Author's comment.

They further opined that explosions below 5-10 kilotons yield could be concealed in Russia, that one megaton would be observed, that long-range detection would *not* give the size of the explosion (to any reasonable accuracy), and that therefore (if a low threshold were to be chosen), close-range surveillance and observers would be necessary.

Lastly, they agreed that a temporary moratorium would not seriously damage the weapons program, but that if it went beyond January of 1957 (2-1/2 years), the detrimental effects would be serious, and that beyond that time the effects would be rapid and cumulative.

By the beginning of 1958, several main paths of effort, with respect to a possible moratorium or test ban, were evident within the weapons complex. While there was real-time interplay between the subjects, they were roughly as follows:

a. information, participation, and "guidance" to the centers of government concerned with treaty-related questions,

b. changes to the test and design schedule to accomplish as much as possible in the time remaining,

c. consideration and actions on those subjects that might maintain the health of the weapons complex post-treaty, and

d. possible post-treaty "readiness to test" considerations.

As mentioned before, the separate components of the complex attacked the problems differently. Briefly, Livermore, still trying to "prove itself" as a laboratory, was hawkish. It emphasized the need of continued testing, warned of possible Russian cheating, proposed alternatives such as underground testing, worried (both theoretically and experimentally) about seismic detection, and pressed for some of those alternate activities that would maintain their competence, such as Plowshare (peaceful uses of nuclear explosives), Pluto (an air-breathing nuclear propulsion reactor system), and testing below an observable threshold. Los Alamos, "old tried and true," took a somewhat more relaxed view. Having been through so many "scares," they really did not believe a moratorium would actually come about, and resisted external pressure to act as if it would. Bradbury and a large portion of his staff thought that a moratorium might actually be good for the laboratory, that some means of coming to agreement with the Russians had to be found, and that further weapon development might not be particularly "cost effective" to the country. LASL seemed to feel that between Rover (space nuclear propulsion program), the compilation and analysis of old test data, and the peaceful contemplation of genuine new weapons concepts, they could be well employed for several years.

The AEC tried to fight a bad situation as best they could. Like Livermore, they resisted a treaty, tried to find alternatives, and urged the accomplishment (at least for the AEC) of as much as possible before such a treaty might come about.

The Department of Defense shared the AEC views and moved in a similar manner. Unfortuntely, AFSWP was in the throes of a possible reorganization (or even deletion) and could not put substantial effort on the subject.

During the year 1958, before the moratorium went into effect, the various test organizations expressed their future needs, through appropriate channels, to the upper echelons, usually with the attitude that they must be ready to test again soon, or that it really wasn't going to happen and life would go on as it had before.

We will now take up separately some of the facets of these activities in 1958, even though the subjects were, in general, not actually separated at the time.

AEC Laboratory Health in the Light of a Possible Moratorium

Partly because of continual questions on the effects of a moratorium, and partly because of just general worrying about the weapons program, Bradbury gave Starbird* some of his feelings on the subject on January 8, 1958. After expressing concern that the laboratory (LASL) had "lost control of its own destiny" since it no longer chose what it felt best to work on in the light of its own knowledge, but rather responded to external pressures from the AEC and DOD (brought about partly by the growing strength of Livermore and AFWSP),** he commented that he felt the laboratories were now making very little progress per dollar invested, and that perhaps a moratorium would be a good thing in a certain sense right now:

> If we had to sit down and think, if we had time to sit down and think, we might think of something. It is very unlikely that with the press of affairs as they are, and with the general attitude of the Commission what it is, and with our own response what it is that we will have the intellectual fortitude to say "No!" to any proposal, nor will we, with the continual workload (which we will partly bring upon ourselves) find the elusive "new" idea if it exists at all. ...A moratorium followed by the possibility of further testing would at least force us to take stock of our whole situation. ...It is my own impression that LASL has let itself get slightly too bogged down in mass production of weapon designs, and that we should try to take that aspect of our life a little easier and work a little harder in general research--which is thought to be good for the country too! It is for reasons like this that the thought of a moratorium, cast in the proper context, is not too painful.

Livermore, however, was not so pessimistic. In March 1958, Teller (who had recently assumed the position of director of Livermore) gave Starbird a thick document listing all of the work required in the major problem areas, and concluded:

> The above enumeration clearly indicates that there is far more useful work to be done than a laboratory of the present size of UCRL can possibly do in the immediate future. This poses the difficult and dangerous problem of choosing the ultimately most useful and desirable ideas from among the many promising and in some cases unexplored candidates. We feel that, at least at the present level, limitations of funds should not be the determining factor in our ability to pursue some of this work.

As a result of the growing pressure, the AEC commissioners called the laboratory representatives into Washington on May 28, 1958, to discuss the effects of a moratorium, but they never got around to the question of the laboratories, spending most of their time on the values of underground testing.

However, Bradbury continued to seek guidance, and it finally came (copy to Teller) on July 11, only a few weeks before Eisenhower announced the moratorium, in a

*Alfred Dodd Starbird--born April 28, 1912--West Point 1933--Army Corps or Engineers--Col. 1944--Instructor, United States Military Academy, 1938-1942--War Department General Staff, 1942-1944--Commanding Officer, 1135 Engineering Construction Group, European Theater, 1944-1945--Operations Division, War Department General Staff, 1945-1950--Secretary, Supreme Headquarters Atlantic Powers Europe (SHAPE), 1950-1953--Office Chief of Engineers, Department of Army, 1953-1955--Director, Division of Military Application, U.S. Atomic Energy Commission, 1955-1961--Director Engineers, Northwest Pacific Division, 1961--Commander, Joint Task Force Eight (Dominic), 1961-1962--Director, Defense Communications Agency, 1962-1966--Director, Defense Communications Planning Group, 1966-1968--Safeguard (Sentinel) System Manager 1968-1970--retired from the Army (Lt. Gen.) 1970--Asst. Director for Test and Evaluation, Defense Research and Engineering, 1970-1975--Asst. Administrator for National Security, U.S. Energy Research and Development Administration, 1975-1978. Four Distinguished Service Medals, Legion of Merit, two Bronze Star Medals. (Ed. note: Deceased 1983).

**Author's note.

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letter from Libby* (acting AEC chairman). The answer was in two parts, as follows:

1. Laboratories as excellent and experienced as Los Alamos and Livermore are national assets and whatever our future holds there will be important work for you to do. Consider two of the possible types of moratoria or disarmament arrangements.

   (a) Test ban only. Then your job--on atomic weapons--would be to digest and collate the results from Plumbbob and Hardtack, which are rich sources of basic weapons science that when fully understood and analyzed will enable us without additional tests to materially improve our weapons designs. A period of eighteen months or two years probably could be most profitably employed in this way. Experimental work at subnuclear yields probably would be involved.

   In addition, we hope that whatever the nature of a test ban, there would be special exception made of the nonmilitary applications of nuclear explosions so this potentially important development could be continued, possibly under the aegis of the test ban authority conducting the inspections and control of the ban. Particularly in the case of Livermore, but also in the case of Los Alamos, this would serve as a meaningful and challenging project to which the weapons design experts might turn their talents to designing Plowshare devices, i.e., devices especially designed for nonmilitary application where consideration of cost, diameter, fission to fusion energy release ratios, neutron escape efficiencies, etc., are dominant as compared to weight, yield to weight ratio, and similar considerations dominant for military applications.

   (b) Full disarmament with present stockpile frozen except for reworking and continued maintenance and Plowshare continued under the aegis of the disarmament authorities.

   The reworking possibilities are large and the full consideration of our present factual knowledge may well reveal significant and important stockpile changes that could be made safely by reworking and without testing at full yields. In any case both the tasks outlined above under (a) would remain.

2. The second part of our guidance would be to advise you to make plans on a strictly confidential basis which you would hold in readiness to reorganize your work and reslot people should a cessation actually occur. The existence of such plans we believe should be closely held by you to prevent there developing in the laboratory a feeling that you, and we, believe a moratorium or cessation is immediate. Neither of us, of course, so believe.

   The plan should be to get the laboratory in the best possible scientific trim beginning immediately so that its ability to perform a wide variety of scientific tasks efficiently and wisely will be at a maximum. Probably the stratification or separation into development groups for weapons or atomic power, etc., on the one hand and into pure research groups on the other which appears to be taking place should be reversed so that the rule would be that all scientists at the laboratory are expected to have research of good quality underway and to be fully conversant with a broad field of scientific literature outside their particular field of development concentration.

   This might lead to more people working on weapons by the addition of part of the personnel from the pure research groups but with everyone being expected to spend part of his time in basic research, the net effort in the development program as a whole would not be greatly changed in total manpower. Of course, there are always individuals who are constitutionally unable to do development work and basic research simultaneously and provisions for exceptions in these cases should be made, but it would be our hope that the shift in trend described be made so that the natural tendency toward stronger and stronger preoccupation with narrow fields and development interest be counteracted so our weapons laboratories can be kept young and scientifically agile. In these ways we think you can plan wisely for the future, whatever it holds.

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*Willard Frank Libby--born Grand Valley, Colorado, 1908--Ph.D. (Chem.), U. of Calif. (Berkeley), 1933--Other Hon. degrees--Staff Berkeley, 1933-1945--Columbia U. War Research Div., 1941-1945--Inst. of Nuclear Studies, U. of Chicago, 1945-1954--AEC General Advisory Committee, 1950-1954--member USAEC, 1955-1959--Prof. Chem., U. of Calif., Los Angeles, 1959 to death in September, 1980. Helped develop gaseous diffusion method of uranium separation--invented carbon-14 dating technique--as Commissioner and as member of the GAC urged Civil Defense, the development of the Super, understanding of fallout radiation hazards, establishment of a second weapons lab. Many awards, including Willard Gibbs Medal, 1958; Albert Einstein Award 1959; Nobel Prize for Chemistry 1960.

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However, the situation changed as soon as the President announced the moratorium (August 22, 1958). On that day the President (Eisenhower) sent the following letter:

> Dear Dr. Teller:
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> I am today announcing that the United States will suspend nuclear weapons tests for a period of twelve months and, under certain conditions of progress toward real disarmament, continue that suspension on a year-to-year basis.
>
> It will, of course, require an extended period to negotiate and install a genuine and assured disarmament arrangement. Even though we will not be doing any weapons testing, it will be necessary that we maintain our weapons development progress during the period and with no less urgency than in the past. It is necessary, in the interest of our country's defense, that the staff of your laboratory, and that of the other weapons development laboratories, continue their research and development in this field with their current vigor and devotion.
>
> I am instructing the Atomic Energy Commission to develop plans to see that these essentials are met and that the vitality of our laboratories is maintained.

Similar letters went to McRae (Sandia) and Bradbury.

John A. McCone, by now chairman of the Commission, emphasized to the laboratories on August 22 that they must maintain the capability to return to testing with a minimum of delay, since the Soviets might not fulfill the conditions set forth by the President for the moratorium. He furthermore pointed out that Plowshare was not included in the moratorium, so that experiments on the peaceful uses of nuclear explosives should be scheduled for firing during the forthcoming year.

In spite of their general support, the Commission worried about laboratory size. The question was apparently triggered off by the growth of the Sandia Laboratory, but the discussion usually concerned Livermore and Los Alamos. Libby had the feeling (early August) that Los Alamos had grown too large, and that Livermore was at just about the right size. Budget reductions because of the proposed moratorium were already being proposed, but on August 27, after Colonel Stewart of DMA* had commented that "the proposed reductions in weapons budget would adversely affect weapons laboratory personnel," the Commissioners stated that "any underruns from other programs would be allocated first to the weapons program." Libby again suggested, on September 17, that the laboratories be held to a limit of 3,000 persons, but no action was taken because of the President's statement that the laboratories should be kept at peak efficiency, and that every effort should be exerted to maintain the morale of the laboratories.

As the moratorium approached, there was time for one more round. In October 1958, McCone requested that the laboratory directors inform him of the status and plans for activities of the laboratories during the moratorium. Teller, for Livermore, replied with their plans to work on Pluto, increase their efforts in pure research, continue with Sherwood (controlled thermonuclear reactors), investigate nuclear weapons using new channels and perhaps methods of testing, study seismic detection with nuclear or high explosives, look at nuclear experiments other than testing, weaponize already proven weapon designs, and expand Plowshare. He pointed out that nuclear explosions might be permitted at high altitude, and that at least theoretical work and nonnuclear experiments should be permitted. Bradbury, for Los Alamos, outlined a program, for a short-term moratorium, of weapons development, improvement in diagnostic techniques, and other means of furthering weapons progress without actually testing, but emphasized that if the moratorium were to continue more than a couple of years the role of LASL in the national picture was not obvious and

*Division of Military Application, AEC.

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should receive very careful consideration at that time. He also pointed out the possible diversion of laboratory effort to Rover, Sherwood, and Plowshare.

Premoratorium 1958 Nuclear Test Operations

It is not here intended to go into any detail on the 1958 test operations, but simply to outline some of the interplay with respect to testing that occurred as the testing community gradually became aware during the year that the moratorium was approaching reality.

At the beginning of 1958, five test operations were in sight. Hardtack, to be conducted in the spring at the Eniwetok Proving Ground (EPG) had been approved for construction by the President, and preparation was well under way. Project 58A was a small operation in the winter at the Nevada Test Site, to include only a few one-point detonations. 58B, soon to be called Millrace, would be a small fall operation at NTS, to include some four Livermore underground tests and several one-point safety tests from both laboratories. Trumpet would be a full-scale operation in the spring of 1959 at NTS, in which Livermore intended to concentrate on underground shots, but LASL would continue its undisturbed way with tower and balloon shots. And planning for Willow, a 1960 EPG series, was just beginning.

Clearly, by this time, Livermore, spurred by Edward Teller, Gerry Johnson,* and others, was well down the path toward going underground for most of their nuclear testing. They had conducted the "Rainier" shot, [redacted] at 1.7 kt underground in September of 1957 and were well satisfied with the results. In early January, Livermore planned to fire a shot at 40-kt yield underground in Millrace, and "By increasing the yields of devices tested by a factor of 20 or so each time, it is hoped to reach the megaton range in underground testing by 1959." Teller was to spend a great deal of effort during 1958 attempting to convince the AEC and the President, with some success, that we could accomplish the main purpose of a test ban, the reduction or elimination of fallout, by going underground. Los Alamos, however, was less than enthusiastic. Bradbury felt that it was most unlikely that good yield measurements could be made underground, or that multimegaton device development could be carried out there.

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In addition to the "normal" AEC development shots, planning had started in mid-1956 by AFWSP, assisted by the AEC laboratories, to include three "high-altitude" shots in Hardtack. The three shots were to become Yucca, a balloon-lifted, [redacted] to be fired at 87,500 feet; Teak, a [redacted] to be lifted by a Redstone missile and fired at 76 km altitude; and Orange, also a [redacted] to be lifted by a Redstone missile and fired at 40 km altitude. The experiments were planned to document the effects of such shots because of the growing interest in antiballistic missile systems. The major portion of the experiments was to be done by the DOD (radar effects, ablation, etc.), but the AEC laboratories would participate (small rockets, nuclear and optical measurements).

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*Gerald W. Johnson--born Spangle, Washington 1917--B.A. and M.A. Washington State 1937 and 1939. Ph.D. (physics) UC Berkeley, 1947. Navy 1941-1946 (Lt. Commander)--Active duty, AFSWP 1951-1953 (participated in Operation Buster-Jangle)--Lawrence Livermore Laboratory, 1953-1961, Associate Director for Plowshare and Test--Test Director NTS mid 50s--Livermore Task Unit Leader, Operation Redwing (1956). Chairman Military Liaison Committee & Assistant to the Secretary of Defense for Atomic Energy, 1961-1963. Returned to Livermore 1963-1966--Director of Navy Labs, 1966-1968--Secretary of Defense Rep. SALT and CTB negotiations 1977-1979. Appropriate Navy and DOD awards.

On January 22, 1958, the AEC approved Hardtack (25 tests) and Millrace (4 tests and up to 10 one-point safety tests), and arranged for execution authority to be requested of the President. Approval for Hardtack was received from the President on January 31, but he did not approve Millrace.

Several complications to the test plans began to appear in January and February of 1958. As a result of the Livermore conviction that "clean" weapons were a boon to mankind, a proposal was made, and accepted by Eisenhower, to include a demonstration "clean" shot (Piñon) in Hardtack. The 14 member nations of the U.N. committee on radioactive fallout were to be invited. They were to be furnished "samples" of the radioactive cloud on which they could do their own radiochemistry. CJTF-7 (Luedecke*) had not yet included this shot in his plans. The idea seemed to be to convince the U.N. that nuclear tests could be conducted without serious fallout hazard to the world, and perhaps that clean weapons would not hurt noncombatants.

Late in 1957, N. C. Christofilos, of Livermore, proposed that electrons from a high-altitude shot such as Teak could become trapped in the earth's magnetic field, and offer a possible AICBM** mechanism, in addition to producing an appreciable amount of radio noise. A long conference, held at Livermore February 10-21, 1958, and attended, amongst others, by J. R. Killian, chairman of the President's Science Advisory Committee, concluded that Teak would not produce serious effects on military radar and radio systems, but that a properly optimized shot might cause difficulties for several months. Because of the large uncertainties in the calculations, the group recommended that a small shot be fired to establish the facts. This was to become Project Argus.

Project 58A had started in December of 1957 with two LASL safety shots. Unfortunately, one of these, Coulomb-C, gave a yield of 500 tons, producing observable fallout on Los Angeles. The project was completed with the Livermore Venus shot on February 23, 1958, and Uranus on March 14.

Further difficulties began to appear. Teak and Orange had been planned to be launched from Bikini Atoll, and construction of the Redstone launch facilities was móng rapidly on Bikini Island during February and March 1958. The question of a possible eyeburn problem had been raised during 1957 planning, but was dismissed as not serious by the DOD planners. However, when the Task Force began to seriously look at the question early in the year, the answer was not so obvious. By March, they were convinced that the eyeburn hazard would extend some 350 miles from Teak, an area including 2,000 to 4,000 Marshallese natives. It did not appear practical to the Task Force to control 4,000 natives over such an area. The alternatives were to cancel the shots, take the chance, or move the launch point. It was estimated that moving would take a minimum of five months. Complicating the problem was the fact that some of the needed measurements were to be made from an Army satellite, launched for that purpose during March, which might no longer operate if Teak were delayed the necessary time to move the launch point. During late March, Starbird urged the

*Alvin R. Luedecke--born Eldorado, Texas, Oct. 1, 1910--B.S. Chem. Eng. 1932, Texas A&M College--2nd Lt. Army Field Artillery Reserves 1932--Wings Feb. 1934 (Kelly & Randolph)--Army Air Force (regulars) Oct. 1938--Military Attache for Air to Central America 1939-1941--U.S. Air Force Jan. 1947--Exec. Sec. Military Liaison Committee to the AEC 1948-1949--Deputy chief AFSWP 1951-1954--Chief AFSWP 1954-1957--Maj. Gen. USAF, Commander Joint Task Force Seven 1957-1958, immediate administrative head of the Hardtack Operation at the EPG--retired from Air Force 1958--General Manager, AEC, 1958-1964--Deputy Director Jet Propulsion Laboratory 1964-1967--Associate Dean of Engineering in charge of research, Texas A&M, 1967-1970--Acting Pres., Texas A&M, 1970--Executive Vice Pres. for Texas A&M system 1970-1976--retired Aug. 1976. Distinguished Service Medal--Legion of Merit (two clusters).

**Anti-Intercontinental Ballistic Missile.

Commission to approve firing the shot on April 16 as planned, trusting to the Task Force to protect the natives. But the Task Force had already proposed alternate sites (Wake, Midway, Christmas, Johnston). Luedecke, JTF-7 commander, discussed the problems with the Trust Territory officials, and Louis Strauss, chairman of the AEC, discussed it with John Foster Dulles. Frank Shelton,* AFSWP chief scientist, Al Graves,** Dodd Starbird, and Herbert Loper, Assistant to the Secretary of Defense for Atomic Energy, jointly agreed (March 2) that it would be wise to move the detonations. On March 22, 1958, "even though he thought the Hardtack test series would be the last in the Pacific and he could appreciate the need for this vital defense information, the chairman (Strauss) questioned the element of urgency, inasmuch as the Commission had known nothing of the development six months ago." In early April, the Commission decided that they liked Johnston Island (Strauss had been there) and told Starbird to seek DOD concurrence on the move and/or concurrence in canceling the shots. Starbird discussed the point with Herbert Loper, who determined, early in April, that the JCS did not wish to delay or move the shots. On April 7, 1958, Strauss and Killian met with State and Defense on the subject. Dulles agreed with Strauss that any case of eyeburn could jeopardize the rest of Hardtack, and hence the recommendation was made to move to Johnston Island. On April 9, the President concurred, with the usual admonition to hurry.

Livermore had proposed their 40-kt underground shot in Millrace as a step toward proving the feasibility of underground testing, but construction difficulties, as they appeared in February and March, made it appear that it could not be accomplished in Millrace, but would have to wait for Trumpet. However, by March, two other pressures were beginning to develop. The Commission was beginning to suspect that the future held only underground testing, if any, so there was need to gain more experience with the technique. Furthermore, the need of further seismic data was becoming apparent. Thus, in early March, both Starbird and Libby argued the need of an early (December 1958-January 1959) test at the NTS of at least 40 kt underground.

While the possibility of a CTB was becoming more real, it still, in early March 1958, was not the only item of concern to the testing system. Over the last several years, there had been a growing feeling, largely fostered by Livermore, that short-time "operations" were not conducive to maximum efficiency in bomb development, and

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*Frank Harvey Shelton--born Oct. 5, 1924--Flagstaff, Arizona--Ph.D., Calif. Inst. Tech., 1953--Sandia Corp. 1952-1955--Armed Forces Special Weapons Project 1955-1959--Kaman Sciences Corporation 1959 to present. Participated in blast and thermal measurements, Operations Tumbler-Snapper, Ivy, Upshot-Knothole. Associated with AFSWP (Kirtland) in preparations for HA event of 1955. Military effects test planning for Teapot MET (1955). As AFSWP Technical Director directed planning and arranged funding for DOD effects tests for Redwing, Plumbbob, Argus, Hardtack I and II. Participated in U.K. Buffalo series, Australia, 1956. Participated in decision to move Teak and Orange from Bikini to Johnston Island. Assisted in White House considerations leading to 1958 test moratorium. Prepared test plans for Willow, including planned high-altitude events. Assisted in formulating high-altitude test plans for Dominic (1962), and participated in tests.

**Alvin Cushman Graves--born Washington, D.C. 1909--Ph.D University of Chicago, 1939--U. of Texas, 1939-1941--U. of Chicago Met. Lab., 1941-1942--Los Alamos 1943-1965. Participated in first nuclear reactor construction and operation at Stagg Field 1942--moved to Los Alamos with first group from Met. Lab. 1943--operated displacement seismographs at Trinity--Group leader M-4 (electric method) 1945--Associate Division Leader M Division 1946--involved in major radiaton accident (over 200 R) while in M Division--Associate Division Leader of temporary J Division for Operation Sandstone (1947-1948)--Division Leader, J Division 1948-1965--Deputy Commander for Scientific Matters (or variations of that title) of Joint Task Forces 3, 132, and 7, Operations Greenhouse, Ivy, Castle, Redwing. Scientific Advisor (or similar title) to the test manager, all Nevada Test Site operations 1951-1965. Deceased 1965. Exceptional Civilian Service Award, Air Force, 1951--Certificate of Achievement, Army, 1954--Distinguished Service Award, FDCA, 1955--Senior Reviewer, AEC--Fellow, American Physical Society.

that "continuous" operations should be considered. Starbird began to investigate this subject by inquiring of the laboratories as to their opinions on continuous operations at the EPG with intermittent underground shots at the NTS, as opposed to continuous operations at the NTS and intermittent large-yield shots at the EPG. Ken Street for Livermore and Bradbury for LASL both preferred continuous operations at the EPG with intermittent operations at the NTS, but Bradbury again expressed his unhappiness at the underground concept for the NTS.

Other evidences of the growing pressure to get problems solved while there was still time appeared in March and April 1958. The DOD, following their growing interest in x-ray effects, were contemplating the design of an underground x-ray simulation shot [redacted] and began conversations with Livermore and Sandia concerning a forerunner experiment that might be done on the proposed [redacted] to establish some of the techniques. Livermore, represented by Gerry Johnson, needed an immediate commitment (April 3), while Sandia simply did not have the effort for a fall experiment.

5 USC 552 (b)(3) Ex. 3, DOE

Now that Teak and Orange were delayed, Commissioner Libby, who was trying to keep worldwide fallout from Hardtack to a minimum, proposed in April that the [redacted] warheads for those shots be replaced [redacted] Bradbury (LASL) replied that [redacted] might be available by 1960.

5 USC 552 (b)(3) Ex. 3, D.O.E

The Air Force proposed (March-April) that systems tests of the Nike Hercules and the [redacted] be conducted as soon as possible. Since it seemed too late to get the shots into Hardtack, they proposed that the shots be done at the NTS. The Commission felt that the NTS was too small to be shooting nuclear weapons at drones, so the Air Force proposed Eglin Air Force Base, which was accepted as a basis for study.

The Argus concept began to take hold. As a result of action by the Armed Forces Policy Council on March 11, Livermore was directed to undertake the necessary further theoretical work and to submit recommendations as to the nature of any nuclear test to be conducted. In order to effect close coordination between the Department of Defense and the Atomic Energy Commission on the subject, the Deputy Secretary of Defense on March 24 designated AFSWP the responsible agency for the DOD, in coordination with the Advanced Research Projects Agency (ARPA). By memorandum April 4, the Deputy Secretary of Defense assigned the overall responsibility for the management of this research and development program for the DOD to the Director, ARPA. During March, the conclusion was reached that it was practical to conduct the experiments, but because of the uncertain future of nuclear testing, the experiments should be done quickly, not as a part of Hardtack, but rather in the Atlantic, with a ship-based launch. The requirement was for 2-10 kt at 500-800 miles altitude and 30^{o}-45^{o} geomagnetic latitude. The Commission approved the concept in principle during April, but worried "that if the shot were carried out at the proposed location, the U.S. would not be in a position to object to weapons testing by the U.S.S.R. in international waters." Teller informed Starbird that there would be no eye damage to observers and, in fact, there would be no perceptible effects at sea level. On May 1, the President approved the nuclear test, to be called Argus, to be conducted before the end of Hardtack, and specifically before September 1, 1958.

On April 28, the first shot (Yucca) of Hardtack was fired. Presidential pressure had led to an initial proposed finishing date of late June, but now, because of the move of Teak and Orange, late August appeared to be the earliest possible end of Hardtack. Additional shots were beginning to appear for Hardtack, but the only further solidification of plans for Millrace was the statement of the intended starting date, September 15. Livermore concluded that by really pushing construction, they might get a 40-kt underground shot ready by November or December, but otherwise the largest shot planned was 5 kt. LASL still planned only a few one-points. The Commission, on April 16, asked its GAC* to consider the question (at their May 5-7 meeting) of how testing might be conducted if only underground testing were permitted. They approved a number of 20- to 40-kt underground shots for Trumpet (early 1959). The crisis had not yet been recognized.

May 1958 was a comparatively static month. The laboratories and field staff organizations were up to their ears in actually conducting Hardtack and preparing for Millrace. LASL had finally broken down (largely as a result of the furor about fallout on Los Angeles from the December 1957 one-point shot) and decided to try some of their proposed Millrace one-point shots underground. Contracts were let to produce the holes (36 inches by 500 feet) between June 8 and July 19, so that, under pressure, operations could start as early as August 1. Tunnel work for Livermore continued. The Commission declassified certain information about Piñon, which was now definite, even though the Task Force had not figured out how to handle the foreign observers. They also approved the Eglin tests, to be conducted by the DOD with AEC review of safety and operational plans. Dulles had stated that such tests should be finished by September 1, in view of a possible moratorium, and Starbird worried that accelerating too many weapons tests to meet a September date might tip our hand internationally, making us appear over-anxious to enter a moratorium. The laboratories requested two definite additional shots for Hardtack and one contingency. The Commission worried about the President's concern with additional fallout [redacted] but concluded that he might accept the additional shots, since Hardtack might be the last test series. They requested (May 28) the additional shots, and also requested approval for Millrace. If Millrace were not to be approved, then they requested two more Hardtack shots, one to be a one-point safety shot. No additional Millrace tests (except for the 40-kt underground) were yet contemplated.

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The picture began to change in June, however. J. B. Fisk, R. F. Bacher, and E. O. Lawrence, now appointed by the President as U.S. delegates to the "Conference of Experts," discussed with Strauss the urgent need of seismic data from a larger underground detonation than Rainier, and were told that such a shot might be possible in October. The field test system started another round of "what can we do?" discussion immediately. Libby (June 12) offered the opinion that some of the disappointing results from Hardtack were coming about because the laboratories were not properly studying data from earlier experiments, but since Hardtack might be the last test series, any tests the laboratories now considered important should be carried out without regard to the number of shots. Starbird moved rapidly, asking the laboratories on June 13 to consider finishing Millrace by November, assuming no monetary limitations. Bradbury, Teller, and Hertford** (ALOO) all answered that they could meet the date, assuming extra funding. By the 18th, the President had approved the additional shots to Hardtack, and had approved Millrace, requesting that it be accelerated to begin before the end of Hardtack. LASL now began to take the moratorium possibility more seriously, and suggested to Starbird that a new set of tunnels to allow some full-scale LASL shots in Millrace might be practical. Starbird said to go ahead, and by the end of June, the decision to start two LASL tunnels had been made, even though it was estimated that the 3,000-foot tunnels could not be completed before December.

Task Force 88, commanded by Rear Admiral Lloyd C. Mustin, was activated for Operation Argus planning purposes June 2, 1958, and for operation on July 14.

*General Advisory Committee

**Kenner F. Hertford, Manager, Albuquerque Operations Office (AEC).

On July 2, 1958, Eisenhower told State to inform Mexico and Cuba of the possible Eglin shots, with the comment that if either government objected, the tests would be reconsidered. The operation was canceled on July 24, 1958.

By now (July 1958), the system was moving in all directions. Commissioner Libby had forced the movement of at least one EPG shot to "reduce" worldwide fallout,* the Piñon shot to demonstrate weapon cleanliness was coming closer, the possible moratorium was becoming more real; and Teller was convincing the Commission to argue for a treaty limiting testing to underground only, rather than a moratorium or CTB. The word was going around that only underground testing would be allowed from now on, and Kenner Hertford (ALOO) proposed that in order to guard against Millrace being canceled, it should immediately be publicly announced as an all underground operation. The laboratory directors and Starbird had a go-round on the subject in mid-July. Starbird had just informed the directors that Trumpet (spring 1959--NTS) would have, in his planning, about 18 shots (Bradbury was worried that Teller would want more than nine, but was uncertain as to whether to argue about it or not, because he was not sure LASL needed even nine). Teller (July 11) felt that DOD, Plowshare, and safety tests should all be separated from weapons tests and that all 1959 weapons tests should be underground (although, were the decision different, Livermore would do a few above ground in order to conduct "special" diagnostic experiments). Bradbury and Graves resisted, but were willing to move toward underground and balloon shots, eliminating tower shots. Starbird (July 17) agreed with both, but would not go along with an immediate limitation to only test underground, suggesting instead "to limit our testing to the degree possible without impeding weapons development." At a lower level, LASL J-6 canceled their tower construction plans for 1959 and began working on six underground locations. Reflecting the now real pressure, LASL tunnel construction was halted (presumably temporarily) since the proposed devices could not be ready before December. Bradbury's information was now (late July) that Millrace would have to be finished by November 15. The LASL test division reaction to all this was relief at not having to rush underground for Millrace, and disappointment at having to do "expensive" testing underground in 1959.

With the additional shots and the move of Teak and Orange, Hardtack was beginning to stretch out, threatening to become a possible embarrassment with respect to a moratorim, and eating into the time that the test experimenters had to prepare for Millrace. Luedecke, JTF-7 commander, who had just been approved as the next AEC General Manager, pointed out (mid-July) that the intended "open" clean shot, Pinon, would stretch the operation an extra two to three weeks. Starbird, on July 10, in Strauss's last few days as AEC chairman, suggested reappraisal of the program. Some 3-4 million of the estimated 10 million dollar cost of the program could still be saved. Six of the fourteen nations invited (Sweden, Brazil, Canada, Belgium, Australia, and France) had accepted, but the Soviet Union, Czechoslovakia, and India had declined. The Commission agreed it should be canceled. But a week later, at John McCone's first Commission meeting as chairman, the subject was chewed over again. The OCB (Operations Coordinating Board), including State and CIA, were all opposed to canceling the shot, on the basis that it would be embarrassing to the President, and that they felt the AEC had not told them all the real reasons for cancelling the shot. The Commission (July 17) changed its mind, agreed that the demonstration should be held, but asked Starbird to try to get Luedecke to accelerate the shot. On July 26, the President canceled the shot.

*During Redwing and Hardtack I, Libby tried to reduce the solubility of radioactive fallout by arranging that large amounts of silica sand be emplaced within the fireball region of Pacific shots and/or arranging that the shots be on a coral reef. If the solubility could be reduced, the hazard of ingestion would be lessened. No effect was noted.

Teak was fired on August 1, 1958, and Orange, the other major high-altitude shot of Hardtack, on August 12. Both detonations occurred at the wrong position in space, and, due to cloud cover, the detailed photographic coverage was almost nonexistent. Hoerlin,* on behalf of LASL, managed to get a request for a repeat of Teak through channels to Starbird even though the DOD said they were satisfied with the results, but the argument was not sufficiently convincing, and (August 14) the request was denied, Starbird agreeing with the DOD decision. (In retrospect, considering the surprises of Dominic, one can speculate on the probably strongly different course of events had the Teak repeat been approved.) At about this time, it became obvious that Argus could not be finished by September 1, and the deadline was extended to October 31.

In spite of all the flurry, in early August the plans for the Millrace operation, to begin September 15 or earlier, still had not changed significantly, Livermore still intending to do a few low-yield shots underground, and LASL intending to do a few one-point safety shots.

On August 18, the last shot of Hardtack, Fig, was fired. Instead of the originally intended 24-25 shots, some 35 detonations took place during Hardtack.

Four days later, the roof fell in. On August 22, President Eisenhower announced a one-year moratorium to begin October 31, 1958. As Bradbury put it, "It was time for the troops to fall out and fall in again." This time the initial question from Starbird to Teller, Bradbury, and Hertford was by telephone: "Give me the possibilities for tests than can be conducted at the NTS before October 31 as soon as possible."** The laboratories, after conferring with ALOO and other parts of the test organization, answered on the same day. LASL had just been given the responsibility for the XW-38, 3,000-pound, [redacted] ICBM warhead, a decision still debatable to Livermore, and proposed to do it by October 10 at the EPG. In addition, they could begin one-point safety tests at the NTS within three weeks, and offered a further list of low-yield devices of interest to the military that might be accomplished by the deadline date. They proposed to fire, in general, in the atmosphere, by any means that could be arranged. On the other hand, Teller answered that Livermore could finish out its work underground. Starbird put it together quickly, and managed to get out the same day the information that Millrace would now be called Hardtack Phase II (for political reasons), that it would include as many as nine small nuclear tests at the NTS, probably one shot at the EPG, and up to seven one-point safety tests. He directed that as many (pertinent) shots as possible be fired by October 31, postponing research, development, and production where necessary. He could not resist, however, telling the laboratories that they must be prepared to reinstate Trumpet at any time, and to continue planning for Willow at the EPG in 1960. The final words were, "We should eliminate projects directed toward conducting operations with greater economy, capacity, or content at either location" (NTS and EPG).

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The next few days saw further solidification. On the 25th, Starbird listed seven tests for Livermore, but showed three as balloon tests, explaining that there

*Herman Hoerlin--born 1903, Schwaebisch-Hall, Wuerttemburg, Germany--Ph.D. Stuttgart--immigrated to U.S. in 1938, naturalized in 1944--Chief Physicist, General Aniline and Film Corp., Binghamton, N.Y., 1938-1953--Los Alamos Scientific Laboratory 1953-1972--visiting professor, Cornell, 1959 to 1960--retired 1972. As Group Leader of J-14 and later J-10, participated in all U.S. atmospheric test operations from Upshot-Knothole to Dominic with the exception of Argus. Concentrated on fireball yield, optical, and high-altitude phenomena. LASL Task Unit Commander for high-altitude shots, Hardtack and Dominic. First ascent of 24,500' Tongsong Peak (Tibet, Nepal) 1930 (highest peak climbed to that date). (Ed. note: Deceased 1983.)

**Inferred quote.

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was no necessity to limit the tests to underground as yet. On the 26th Bradbury reaffirmed LASL intent to do the ICBM warhead at the EPG, but warned that the date was already slipping. On August 28 the President approved an accelerated Hardtack Phase II, but disapproved any further EPG shots, wiping out the XW-38 test.* The next day, McCone and McElroy (Secretary of Defense) publicly announced Hardtack Phase II, describing it as about ten low-yield nuclear detonations, several of which would be underground. But Sandia** was already moving rapidly to prepare for balloon shots.

The first Argus shot was fired August 27, 1958, not particularly satisfactorily, being followed quickly by the second on August 30, and the third on September 6. The "Argus" effect was not so serious as feared.

The Nevada Planning Board met on September 9, 1958, at Mercury, Nevada, being chaired by Duane Sewell*** of Livermore. The plan for Hardtack Phase II discussed was for six tunnel shots, one tower shot, up to four balloon shots, and several one-point safety shots. Among the agreed upon assignments were: Jim Reeves, Test Manager; Gerry Johnson, Deputy Test Manager; Duane Sewell, Scientific Advisor; Col. W. S. Hutchinson, Deputy for Military Matters.

Hardtack Phase II was a wild operation. It began September 12, three days before the earlier planned date, with a LASL one-point safety shot, Otero. Instead of the earlier planned four underground and seven one-points, there were 37 detonations in all. The largest underground detonation was Blanca, at 19 kt. The detonations took place underground, in the air (balloons), on various height towers, and even in "Gravel Gerties." The laboratories even traded firing sites on occasion to move faster. The last shot was Titania, a Livermore detonation, at 1:34 p.m. on October 30, and one shot, Adams, was left hanging in the air as midnight, local time, went by. The period was one of continual changes, requests to the President, DOD proposals, etc.

But the operation seemed hard to kill. On October 28, since the Soviets had indicated that they might continue their testing beyond October 30, Starbird asked the laboratories for plans to continue testing beyond October 31, what could be done in three months, six months, etc. Bradbury (LASL) came close to rebellion, pointing out that it was time to quit for a while and survey the situation, politics or no politics. He did weakly mention a few things that could be done, if really necessary. The October 31, 1958, meeting of the AEC resulted in the following note:

[redacted] However, in the afternoon, when the test was scheduled, the atmospheric conditions were such that some blast damage would have occurred over the Las Vegas area and so the test was delayed. At 11:00 p.m., the

5 USC 55. (b)(3) Ex. 3, D.O.E.

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*The W38, reassigned to Livermore, entered the stockpile in 1961-62 and was retired in 1965. The W38 was never tested.

**Sandia Laboratories, Albuquerque.

***Duane C. Sewell-born Oakland, Calif. 1918--Graduate student under E. O. Lawrence at Berkeley 1940--Manhattan Project, Oak Ridge (Y-12) During WWII--Assisted in development of 184" cyclotron at Berkeley, 1946-1950--MTA accelerator development of Berkeley 1950-1952--Became Director of Scientific Operations of UCRL (Livermore) in 1952--Senior operational member from Livermore for Operation Upshot-Knothole, 1953--Managed Livermore's nuclear test operations for Castle (1954), Teapot (1955), and Redwing (1956)--Scientific Advisor to the Test Manager for Hardtack, Phase II (1958)--Associate Director of LLL for support, 1959--Deputy Director LLL, 1973--Asst. Secretary for National Security, DOE, 1977 to 1980. U.S. AEC Citation, 1971--ERDA Distinguished Associate Award, 1977.

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weather seemed to be clear and an additional high-explosive test was held to determine the blast prediction. This indicated that the last shot could safely be fired and a test was scheduled for 11:30 p.m. and General Starbird said he approved proceeding with the test at that time. Subsequently, the weather changed and it was decided the test would have to be delayed until 2:00 a.m. Starbird said he then conferred with Department of State officials and was told that U.S.-U.S.S.R. discussions were scheduled to begin in Geneva at 9:00 a.m., EST, October 31. He stated he decided at this point that in view of the probable political and psychological repercussions of holding a test at this late date the final test should be canceled. Starbird said there will always be a question as to whether the final shot should have been fired in view of its importance [redacted] but that he believed that the other considerations were of overriding importance. The Commissioners unanimously agreed that General Starbird had made the right decision in canceling the final test.

5 USC 552 (b)(3) Ex. 3, D.O.

Plowshare

Concepts concerning the possible use of nuclear explosives for nonmilitary purposes were discussed even before the first successful nuclear detonation. However, the program really began to move in the late 1950s with the establishment of the *Plowshare* (or PNE, Peaceful Uses of Nuclear Explosives) program, largely pressed by the Livermore Laboratory (then UCRL). Since the program grew at the same time as the worldwide pressure to ban nuclear weapons tests was growing, several emotions contributed to its approval. The program, if successful, would counteract the fear of nuclear detonations to some extent. It would (or would not, depending on the debater) allow some investigation of nuclear explosive design, especially clean design, under a nonweapon guise. It might actually be of some value to the human race. But most important, in the light of the subject of this book, it might be helpful toward keeping nuclear explosive design and experimental work continuing during a moratorium or test ban period.

In early 1958, whatever the reasons, Livermore and DMA were attempting to expand the program, with some help from Sandia, but essentially no interest on the part of Los Alamos. The main promoters at Livermore were Edward Teller and Gerry Johnson. Agreement had been reached with the Commission that the first attempt would be to conduct a harbor excavation experiment near Point Barrow, Alaska, in mid-1959. A four million dollar 1959 budget was approved in April (1958) for that purpose. In addition, conceptual planning was beginning for industrial application tests directed toward power production, mining, and isotope production. By May, the estimated cost was already up to seven million, and Starbird was looking, without success, for additional funding from the Departments of Interior and Defense. By June, the harbor project had been named "Project Chariot," no site had been chosen, but the detonation was now delayed to the second half of 1960. A second definite project, "Gnome," at 10 kt, to investigate power production, was now planned for early 1960 in New Mexico.

The August 22 announcment of a moratorium engendered a strong defense of Plowshare. On August 28, Teller wrote to Eisenhower and McCone, "All of us are anxious that the great possibilities of using nuclear explosives in peaceful pursuits should be fully exploited. We feel that if we do not succeed in carrying through this work, the United States will, in the long run, suffer in its power and its influence in a decisive manner." McCone answered the next day, " . . . The Commission believes that Livermore and LASL should give a high priority to this project. . . . Useful experiments can be scheduled . . . during the year's suspension (October 31, 1958-October 31, 1959) as well as for later periods. I request that your revised program be submitted. . . ."

By October, Teller had convinced the Commission that Plowshare work should not be confused with nuclear weapons work and had increased the scope of Livermore studies to include the following items:

a. a channel through the reef at Kapingamarangi,
b. a harbor at Cape Thompson, Alaska,
c. a harbor at Katalla, Alaska,
d. a sea-level canal across the Alaska Peninsula at Port Moller,
e. oil excavation for Tar Sands,
f. a second power and isotope production-type shot,
g. the creation of artificial aquifers,
h. mining by leaching, and
i. excavation of oil from oil shale.

He further assumed that Gnome would be fired in FY 1959, and that three other experiments would be conducted in FY 1960, all to cost 5 million dollars in FY 1959 and 14 million dollars in FY 1960.

In mid-October, McCone suggested that the Operations Coordinating Board establish a Plowshare Advisory Subcommittee to stimulate wider interest in the program. But by the same time, the question of how to conduct "open" Plowshare shots under a treaty, convincing others that these were not really weapons tests, and still not revealing weapons design data, had already raised its ugly head. No immediate answer was obvious. McCone commented, however, on October 15, that "any competent scientist in the weapons field could determine simply from observing the instruments whether they were intended to record a weapons or a Plowshare test."

Nevertheless, Livermore entered the moratorium with this active, apparently funded, program that could "legally" keep some of their design and experimental people busy for a while.

Low-Yield Testing

Another concept that showed some initial promise of helping the nuclear weapon design and testing community came up not long before the moratorium began. Again, the question was raised by Edward Teller, who was convinced that the Russians would cheat, if possible. He therefore argued that, in essence, any test that was not detectable should be legal. On August 29, 1958, he wrote to Starbird that, as a general rule, any experiments with designs in which the nuclear energy production was not more than the energy production by the high explosive were obviously not tests of the nuclear weapons and should be permitted as experiments. Furthermore, since tests of a kiloton and smaller could not be detected and identified, he suggested that "explosions of military significance below a limit of at least 100 tons be permitted. These explosions will be important for our future weapons development." He further suggested that any future international agreements should not prohibit tests, but should simply put a limit on the effects. During this time, he also made the point that one-point safety shots could not be considered nuclear tests.

Starbird answered (September 4), agreeing that one-points were not tests, that planning should continue to conduct such experiments, but that Presidential approval would be required. Yields would have to be limited to a few pounds or less. However, he felt that announcing that 100-ton and below tests were legal would not be politically acceptable, although he would take it up at a higher level. Starbird followed up, and informed Teller and Bradbury a little later that the U.S. would strive in the forthcoming negotiations for authorization to conduct safety tests up to a yield of 10 tons, and hydrodynamic tests with nuclear material, but producing zero yield. The idea was that the safety tests might produce a small nuclear yield, but would just be conducted for safety reasons, not to develop new weapons. The hydrodynamic tests might be valuable to weapons development, but would not produce any yield.

Bradbury commented (October 13) that a one-point test resulting in a nuclear explosion of any yield would violate the spirit of the President's moratorium, that diagnostically one-points could not be distinguished from any other explosion so that policing would be essentially impossible, and "From the general philosophical point of view, we believe that if a moratorium is worth entering at all after considering the balance between technical loss and diplomatic gain, this balance will not be in the least changed by the trivial addition, even if one knew how to enforce it or make it effective, of a one-point safety test program." Furthermore, he suggested that the question of hydrodynamic tests not be brought up at all, since they would produce no nuclear yield, but "we, of course, intend to pursue weapons development by any means we can which does not involve nuclear explosions."

On October 16, Teller included in his laboratory plan one-point safety shots and experiments using nuclear materials but not leading to a nuclear yield.

While the argument shows in the higher-level record only late in the game, Livermore had done their homework. They had early in 1958 requested the AEC in Las Vegas (Max Smith) to study the design of a vessel to hold an explosion as large as 300 pounds of H.E., with provision to recover active fissionable material. Appreciable work had been done on the design by the time of the moratorium.

Teller raised similar points during this period with respect to undetectable deep space testing. Were one-points to be allowed during the moratorium, many of the test capabilities could be exercised and maintained.

Physical Test Readiness

As might be expected, actual moves toward establishing a postmoratorium readiness to test were rare up until the time (August 22, 1958) that the President announced the moratorium. After all, there were at least two proving grounds in operation, competent people were already in the system, and the moratorium ground rules had not been established.

The first formal moves came, not surprisingly, from within the testing organizations themselves. In early May 1958, AFSWC (Air Force Special Weapons Center) began to prepare a plan for their operations in the event of a moratorium. AFSWC furnished the major effort for TG 7.4, the Air Task Group of the Joint Task Force operating at the EPG. They also furnished air support for NTS operations. They (Col. James F. Crosby) concluded that their job would be to support operations at either test site on six-months notice, and therefore the 4950th Test Group would reduce to half strength, and keep its space. The 4926th (sampling) would be needed to monitor possible foreign tests, and hence would stay at full strength. The 4951st, at Eniwetok, would have to maintain capability at Eniwetok and hence would stay at full strength. The 4935th (NTS) would stay at full strength for similar reasons. The 4952nd would be reduced to one office and one man. Little did they know!

Col. Wignall (Deputy Commander, Task Group 7.4) worried (May 19) about even this much reduction, at least for the first six months, and suggested that some effort could be used preparing a detailed record of the procedures developed over the years of testing. Col. Kieffer, Commander, TG 7.4, had digested all this by July 30 and recommended to Luedecke, Commander, Joint Task Force 7, that no reduction below normal testing interim levels be accomplished, on the assumption that a six-month readiness after November 1959 would be required.

Barney O'Keefe,* at Edgerton, Germeshausen, and Grier, Inc., proposed (June 17, 1958) a basic policy to the company. He first stated, giving the appropriate political reasons, that it was a virtual certainty that a moratorium would be declared, and assumed October 1, 1958, as the magic date. He then predicted that the contractors would be told to maintain a six-month readiness to test, with the immediate consequence that the AEC would stop procurement on items of less than six months lead time, and would insist that personnel in slots that could be filled in sixty days or less be fired. He further assumed that facilities required for the test program could no longer be justified. He then proposed an 11-point program to meet the situation, including restricting hiring and facility procurement, developing alternate programs, and vigorously entering into a readiness program, assuming Starbird's and Hertford's cognizance of their situation. The plan was followed, and in 1961, EG&G was there to help.

CTG 7.2 (U.S. Army Col. Stanley Sawacki) suggested to Luedecke (August 4) that TG 7.2 also would need its normal interim joint table of distribution if a six-month readiness after November 1959 were required. But he also suggested that TG 7.2 be eliminated, with its functions being picked up by other Task Groups. His interim joint table of distribution was 1,100 personnel.

The Air Force Scientific Advisory Board, the membership of which included Harold Agnew, John Foster, Dave Griggs, Al Latter, and Edward Teller, took up the subject on August 8, 1958. They recommended, "Planning for future tests should be conducted intensively and with periodic reviews." They did not mention funding for the field organizations.

The subject got a lot more attention on and after August 22, however. On that date, Starbird started down the path that was to so infuriate the laboratories over the next three years. Stating that we should be prepared to revert to testing on short notice if the situation warranted, he went on, "We should be prepared to reinstitute Trumpet at NTS *limiting major expenditures to those essential to readiness, and approved individually by DMA*,** and include in our plans the possible conduct of a spring 1960 series in the Pacific. Our budget should be based on and tailored to such an approach." McCone wrote to Teller (August 29), "Your efforts should be so oriented that, in the event the test suspension is not extended or is canceled, we can revert to testing and ensure consequent advancement of our developments with a minimum of delay."

On September 8, Ogle (then Scientific Deputy, JTF-7) wrote to Luedecke, defending the continued participation of military personnel in Task Group 7.1. Over a hundred people were involved.

Other parts of the system began to respond, in spite of the pressure of testing. Jim Reeves,*** Nevada AEC, met with Holmes & Narver, Inc., on September 19 to help

*Ed. note: Bernard J. O'Keefe has been Chairman of the Board, EG&G, Inc., since 1972.

**Emphasis added.

***James Edson Reeves--Born Atkinson, Illinois, 1906--M.S. Hydraulic Engineering, Univ. of Iowa, 1930--Army Corps of Engineers (civilian) 1930-1952 except for a year (1944-45) at Tennessee Eastman (Oak Ridge), Mississippi River nine-foot channel 1930-1938; third lock for the Panama Canal 1938-1942; trans-isthmus sea-level canal 1942-1948; Greek rehabilitation projects 1949; flood control, navigation, and military construction in the Pacific northwest and Alaska 1949-1952--Deputy Director, Office of Test Operations, Albuquerque Operations Office of the AEC, 1952-1953--Director of the same office 1953-1957--Assistant Manager for Field Operations, Albuquerque Operations Office of the AEC, 1957-1962--Manager, Nevada Operations Office of the Atomic Energy Commission, 1962 to Dec. 31, 1968--Assistant Test Manager, Upshot-Knothole (1953)--Test Manager, NTS, 1955-1968--Participant, Operation Ivy, Eniwetok Proving Ground (1952)--Commander, Task Group 7.5, EPG, 1955-1958--Commander, Task Group 8.5, Dominic, 1962--Retired Dec. 1968--Army Certificate of Appreciation 1954--AEC Honorary Superior Performance Award 1959--AEC Distinguished Service Award 1961.

them put together a "state of readiness" plan for Eniwetok. Based on ALOO guidance, they planned for a capability to resume full-scale testing in nine months, and developed the costs, H&N personnel, and procurement necessary. But the situation was still confused. Reeves wrote to Graves (September 29) that, "As you can undoubtedly appreciate, we are in a state of considerable confusion regarding the degree of preparedness for testing which we will maintain following the moratorium, if it comes to be on October 31." Starbird had given him, verbally, three criteria:

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But he (Reeves) pointed out the second major difficulty of the next three years by commenting that the Bureau of the Budget was already tying up most of the construction funds for the two test sites, and putting on pressure to reduce the maintenance and operations costs.

Eisenhower sent a message to Congress on April 3, 1958, proposing reorganization of the Department of Defense. For the next five months, AFSWP was busy trying to help define their own future, and had little time to consider "readiness." They did their homework well enough that when asked for their views on August 6, they responded within a week with the plan that was to lead to the eventual establishment of their follow-on agency, DASA (Defense Atomic Support Agency).

Nevertheless, on October 1, 1958, Chief AFSWP (Rear Adm. E. N. Parker), forwarded his intentions to the Assistant Secretary of Defense (R&E) as follows:

a. AFSWP intends that test planning during the suspension period be directed towards conducting an overseas operation first, when the suspension is lifted.

b. AFSWP has requested authority to expend funds from $2,000,000 already allocated in the FY 1959 budget for preliminary planning for Operation Willow. AFSWP has tentatively estimated that an additional $4,000,000 will be required in FY 1959 for preplanning Willow.

c. *First-priority high-altitude requirements:* [redacted]

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d. *Second-priority high-altitude requirements:* Desirable, but of lower priority are:

ALTITUDE	DEVICE	APPLICATION OR REASON
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He further suggested that interest would be shown in another set of experiments, which he had not yet coordinated with the services, as follows:

a. *First-Priority Requirements*

[redacted]

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5USC552 (b)(3) Ex. 3, DOE

b. *Second-Priority Requirements*

[redacted]

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c. *Third Priority*

[redacted]

One of the earliest moves toward a different way of thinking came when Don Shuster* (Sandia), then Commander, Task Group 7.1, wrote to Luedecke recommending a captive balloon shot facility at Engebi (EPG) to reduce the costs of maintaining a readiness capability and to shorten the time from notification to operational status. LASL and Livermore were not particularly enthused. Luedecke forwarded the suggestion to the JCS and to McCone with the recommendation that the capability be developed and maintained during the moratorium. Starbird asked ALOO to consider the proposal, and provide funding estimates if they concurred.

Luedecke, soon to be General Manager, AEC, asked Ogle (October 21) to comment on his intended recommendations to the JCS and AEC concerning the possible capability to resume nuclear testing. Luedecke first reviewed the political situation, commenting along the way that:

> Our experience indicates that the U.S.S.R. will resume testing at such a time as the Kremlin considers that it is in their best interests to do so, progress of negotiations or agreements notwithstanding. However, it appears possible, or even likely, that their interests would best be served by cooperating in negotiations to the extent necessary to cause the United States to refrain from testing for an extended period of time.

He proposed that the AEC and DOD could maintain a capability to conduct a limited number (3-4) of proof tests at both test sites within three months, and 10-12 developmental tests within nine months, if (a) continuous plans were maintained; (b) continuous capability to activate a test organization were maintained; (c) necessary

*Don B. Shuster--born 1921, Santa Fe, N.M.--Attended New Mexico Military Institute--U.S. Army, 1941-1946--joined Sandia Laboratories, 1946--Manager, Instrumentation Department, 1951--Full Scale Test Department, 1955--Director of Field Testing, 1959--Director, Aerospace Programs, 1963--Director, Special Projects, 1965--Director, Advanced Systems Development, 1966--Director, Exploratory Systems Development, 1968--Director, Exploratory Project, 1973--Director, Advanced Planning Analysis, 1973--Director, Exploratory Weapon Systems, 1975. In charge of High Resolution Telemetry, Operations Ranger and Greenhouse (1951)--Cryogenic Monitoring Instrumentation, George shot, Operation Greenhouse--High Resolution Telemetry, Operation Buster Jangle (1952)--Commander, Sandia Task Group, Operation Redwing (1956)--Associate Test Director, Operation Plumbbob (1957)--Commander, Scientific Task Group (7.1), Operation Hardtack (1958)--Deputy Scientific Deputy, Joint Task Force Eight, Operation Dominic (1961-1962).

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plant, equipment, and funds were adequate; (d) provisions were made for "normal service support" by appropriate AEC and DOD agencies; and (e) studies were conducted of alternate means of conducting test operations to effect simplification and economy. He went on to recommend that the JTF-7 responsibilities be assigned to AFSWP (he came directly from AFSWP), who would work closely and continuously with the AEC on these subjects. He recommended deactivation of JTF-7 and its subordinate units. EPG would be taken care of by the AEC. He recommended that the U.S. maintain a capability to test within three months.

Ogle could not see the broader points, and could not stand the idea of AFSWP being responsible for future test planning (they might not even exist six months later). He proposed that the important items were the maintenance of the AEC laboratories and AFSWP, the proving grounds, and the appropriate communication channels, but that a central active planning organization was of secondary importance and, in fact, would bore the people "involved in the continuous and thankless job of maintaining "war plans" that must be changed continually and may never come to fruition." He strongly urged a point of view that AEC diagnostic measurements were up to the AEC and its contractors, and were not within the cognizance of AFSWP. Two years earlier, Luedecke as Chief, AFSWP, had been trying to convince Ogle that AFSWP measurements were none of the AEC's or Task Force's business. Depends on your point of view.

On September 19, 1958, the Secretary of Defense promulgated the guidance that limited test operations might be initiated by February 1960, but that extensive test operations would not be initiated before mid-1960. Following that guidance, Chief, AFSWP (October 26), requested funding to continue Trumpet projects and to initiate a complete moratorium weapon effects program. Any Trumpet agency in need of immediate funding to prevent collapse was requested to submit details and would be provided assistance as soon as possible. AFSWP would develop a complete and comprehensive moratorium period program as soon as feasible.

[redacted]

5 USC (b)(Ex. 1 D.O.

Within a week (October 28), the program had been laid out. [redacted] Apparently the contractors could. Just the day before, Nevada AEC had called a meeting of its contractors (H&N and REECo) at the NTS-CP to discuss their readiness. In addition to Reeves and Bill Allaire of the AEC, Sam Howell (H&N), Lew Reynolds (REECo), and Carol Tyler (REECo) attended. The guidance was offered that capability to resume testing promptly would be maintained, continuity of personnel would be required for balloon handling crews, microbarographic and seismic measurements, B and E tunnels would be reopened, etc. In addition, the following general guidance was offered:

5 USC (b)(Ex. D.O

5 USC (b)(Ex. D.O.

(a) DMA (Starbird) has requested that we clear with them on any major engineering studies or programs which might be initiated concerning future test activities; (b) if and when testing at NTS is resumed we should assume that the tests would be on a continuous type basis rather than the short operational periods which have occurred

~~SECRET~~

in the past; (c) that any future test program would probably involve a heavy diagnostic effort; (d) that during any interim period it is contemplated that there would be periodic meetings of the Planning Board on about a three-month interval; (e) that there would be a relatively large effects effort, both DOD and civilian, in any future test program; (f) that it would be entirely possible that criteria would be developed during the interim period with the result that such criteria would be dropped into the laps of the architect or the construction contractor practially overnight when the decision to resume testing was made. This would result in a high abnormal work load.

Starbird, as Director of the Division of Military Application, closed out the period nicely in his October 31, 1958, message to the General Manager, AEC, on readiness. In reviewing the political situation, he commented:

The danger to our national security lies in the strong likelihood that the U.S.S.R. will protract negotiations and "cooperate" only to the extent necessary to cause the United States to refrain from testing for an extended period of time.

He went on:

. . . our readiness to resume testing, should the President so direct, must be adequate to permit the following:

[redacted]

5 USC 552 (b)(1) Ex. 1 DOD

The maintenance of a capability to resume testing on the above time scale will require, as a minimum, the following:

THIS SECTION WITHHELD UNDER 5 U.S.C. 552(b)(1) EXEMPTION 1, D.O.D.

Finale

And so, the testing community entered the moratorium with some optimism. The President, the Secretary of Defense, and the AEC had all indicated their support of a strong and viable readiness program. It appeared that Plowshare detonations, one-point safety shots, and conceivably even shots with "just a little" yield might be allowed. Rover and Pluto could continue. There were lots of data to be analyzed, and time to do it was welcome. In fact, to most testers, the moratorium was welcome. The testing system was tired, tired, tired. Duane Sewell pulled down his balloon (not without some trouble), and everyone went home.

CHAPTER II

TEST MORATORIUM, 1958-1961

Test Organization Situation, Late 1958

But the indefatigable Starbird did not rest. On the 12th of November, 13 days after the moratorium went into effect, Starbird presented a coordinated weapons test readiness program to the Commission*. Luedecke, as Task Force Commander, had already presented his recommendations to the Chief, AFSWP, the Joint Chiefs of Staff, and the Atomic Energy Commission**. At this time there was a strong dichotomy between the two weapons development laboratories. Livermore, inspired by Teller, was enthused about underground testing, was pushing Plowshare, and was beginning to push experiments that might have to do with establishing the characteristics of seismic signals from underground detonations. The Livermore staff were fighting desperately for future nuclear device testing, either underground under the auspices of Plowshare, in deep space, or any other way that could be found. Los Alamos, on the other hand, was not anxious to test. Norris Bradbury and a fair fraction of his staff genuinely believed a moratorium, or a later test ban, might be to the benefit of the United States and, perhaps even more broadly, to the benefit of the world. Bradbury did not believe that a capability for an immediate testing response was important. As he put it, "It takes four years to get from test to stockpile. What do a few months matter?" His advisors felt strongly that underground testing was a difficult method of testing devices. The diagnostics would be uncertain, the costs would be high, and high-yield device tests would be too expensive. This difference in attitude is reflected in the correspondence of the period; Livermore offering the possibility of great advances in yield-to-weight ratio, clean devices, etc., if testing were allowed; Los Alamos, largely in the persons of Bradbury and Carson Mark, trying gently to refute some of the claims, but leaning more, always, in the direction of some sort of international limitations.

It is pertinent to point out here that there were genuine weapons problems immediately after the moratorium went into effect. [redacted] Furthermore, as Libby had commented, there was a tremendous amount of data to be analyzed. Unfortunately, in 1958, the computer capability in each laboratory was not really sufficient to take proper advantage of the data presented. The data analysis from Hardtack Phase II in Nevada did not take long, but there were difficult problems in understanding the results from secondaries tested in Hardtack Phase I. The effects data from Teak and Orange were fragmentary

5 USC 552 (b)(3) Ex. 3 DOE.

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*Essentially the same as his October 31 plan, but including the aborted "Adams" event of Hardtack Phase II.

**See the section "Physical Test Readiness" near the end of Chapter I.

and incomplete, so that analysis was extremely difficult. The point of this chapter is to explain how the testing organization kept, or did not keep, a capability to test in the future. Let us examine the question: Where is the nuclear weapon testing capability? At any time. Either in 1958, 1959, 1961, or 1975? It is in three places. Livermore, for Livermore devices and Livermore-sponsored Department of Defense tests; Los Alamos, in the same way; or DASA, through its contractors. The rest of the systems do not furnish testing capability but furnish the surroundings by which the testers may do their work (with certain exceptions having to do with systems tests or NUTEXs). The capability to make the measurements that are the results of development tests and are the only proof of a successful test lies, in 1958, 1961, and today, in the weapons laboratories, Sandia, DNA, and their subcontractors, where the major technical subcontractor is EG&G. The AEC has had nontechnical contractors in the past, as has the DOD. These contractors have contributed tremendously to the success of our previous weapons tests, but they have not been essential in the sense that the two weapons laboratories, LASL and Livermore, and then Sandia and EG&G, have been. At this point, it seems pertinent to digress to these five prime organizations--Los Alamos Scientific Laboratory; Lawrence Livermore Laboratory (then UCRL, Livermore); Edgerton, Germeshausen & Grier; Sandia Laboratories; and Defense Atomic Support Agency. One can regard these as the five primary organizations for nuclear weapons testing, and put as secondary such organizations as Holmes & Narver, ALOO, the Task Force, and various DOD contractors. In the question of weapons development and the diagnostics thereof, before 1958, Los Alamos was clearly the senior organization and almost the controlling organization. However, UCRL made important contributions from the very beginning, became very strong in the subjects as early as 1952, and became a serious faction in 1955. Sandia, originally an offshoot of Los Alamos, contributed continually and strongly to the methods of carrying out an operation; but the basic AEC objectives, with very few exceptions through 1958, came from the weapons design laboratories rather than any other organization. EG&G, a profit making concern, had been formed at the request of the AEC in the late 40s to furnish technical support to Los Alamos (later expanded to include other weapons laboratories). In the period before 1958, EG&G was essentially an equal partner in testing with the weapons laboratories (perhaps a little more equal with Los Alamos). Other organizations were of secondary importance. NVOO was still a field office in ALOO and while its guiding light, Jim Reeves, under the auspices of Kenner Hertford, was terribly important, his efforts were still subsidiary to what the laboratories were trying to accomplish. In the DOD, the situation was a little different because the basic technical competence was not in AFSWP (DASA) but in its contractors.

So the problem at the beginning of the moratorium was simple from the point of view of the laboratories: How is competence kept alive? They assumed there would be another operation of some sort, and the problem was simply to battle the AEC, the Department of Defense, the OMB, and, ultimately, the President to arrange for that funding necessary to support the efforts of maintaining the competence. Any framework that would accomplish this funding and at the same time engender in the personnel involved a sense of mission would be satisfactory. Obviously, such a framework would be better if it were clearly meaningful.

The three basic AEC weapons laboratories had differing problems. At Los Alamos, contrary to the author's feelings and to a certain extent to those of Al Graves, Norris Bradbury had no interest in Plowshare. So Los Alamos had practically no input or effort involved with that subject. The Test Division did have some effort, but it was very small, perhaps 5% of the Division effort, involved with surface-based detection of high-altitude explosions, and that kept a few people busy. But the main objective was to prepare for further nuclear tests. The directives from Washington were continually changing, never very consistent, but always supportive of whatever the laboratories could put forth to maintain some capability. The engineering support was maintained at Los Alamos, at least for the first half of the moratorium, because of the decision by Bradbury to make the Test Division responsible for nuclear propulsion reactor testing. Thus, the operational people and the civil and electrical engineers could be given a good profitable job to do that would exercise their talents but still make them available if weapons testing were to occur again. More critical problems were in the test design groups. Let me take those roughly one at a time.

The radiochemical group had a great deal of work to do analysing samples left from previous operations, including foreign tests, and they had many problems on the basic physics of fission to keep them busy. The Rover nuclear reactor tests contributed strongly to the maintenance of their capability. They could exercise their cloud phenomena theories, their sampling theories, and most important of all, their continued data analysis capabilities. The group that previously had been concerned with the neutron outputs of nuclear weapons now became involved with neutron outputs of nuclear propulsion reactors, and with the design of future experiments that would allow more detailed observations of a thermonuclear burning region. The group concerned with the measurement of reaction history completely changed its character during the period of 1958 to 1961. It was split up into two sections, one concerned with some of the detailed characteristics of nuclear propulsion reactor observations, and the other very senior technical capability that was left over remained in an essentially advisory capacity to the Division Office. The capability to measure the reaction history was in serious jeopardy for some time at Los Alamos.

The Los Alamos group concerned with the measurement of fireball characteristics had many problems to work on during the moratorium, in fact, so many problems that mundane testing problems got in their way. Basically an astrophysics group, they could and did spend time refining the fireball expansion theories so as to explain the past discrepancies between fireball and radiochemical yield results. Time was now available to attempt theoretical confirmation of previously measured radiation opacity values.

The group that had been concerned strongly with very detailed esoteric measurements of the internal workings of thermonuclear devices found this period terribly traumatic. Their measurements in previous operations had been regarded with great interest by the theoreticians but had not been particularly useful because the calculational techniques available did not allow the experimental results to be put into the theories of the weapons of those days, and thus, support to the group during the moratorium was not overly enthusiastic, and its morale by the end of the moratorium was very poor. However, they did study the characteristics from past records of electromagnetic effects and similar phenomena.

The group at Los Alamos that had been concerned with blast and optical phenomena took on the principal responsibility at Los Alamos for Vela Sierra, the name used to identify work on surface-based detection of atmospheric and high-altitude explosions. They designed and assisted in the construction and operation of the AFTAC systems for observing atmospheric detonations.

Los Alamos had always depended for its testing capability not only on internal competence, but on that of external contractors such as the Naval Research Laboratory, Naval Ordnance Laboratory, EG&G, the National Bureau of Standards, and others. Their difficulties will be mentioned later. However, the Test Division in Los Alamos also depended very strongly on the support of the other divisions within Los Alamos, mainly the Physics Division, from which both the Division Leader and the Alternate Division Leader had come. That Division, which had furnished people such as Louis Rosen, Austin McGuire, and Keith Boyer, could, for at least a while, maintain its strength within its normal charter of carrying out the physical research for the Laboratory. In addition, P Division, with some help from the Test Division, took on the job of satellite-borne high-altitude and deep space test detection. Other assisting Divisions, such as CMB, also had normal charters which would support their people.

At Livermore, the situation was somewhat different. The background of test information to be analyzed was not so extensive because Livermore had been formed comparatively recently. They were tremendously affected by Edward Teller, who was determined now as Laboratory Director to maintain his Laboratory's capability to resume testing under any circumstances. Livermore had gotten strongly involved in a number of real time issues that became very helpful to them. Not only was the Pluto air-breathing nuclear propulsion reactor in full flower, but they were the main exponents of the use of nuclear explosions for peaceful purposes (Plowshare), and very quickly picked up the major effort within the AEC on the subject of seismic detection.

Thus, Livermore had a large advantage, in principle, over Los Alamos. They had started underground tests and had tunnel designs and configurations. They had an ongoing funded program in Plowshare, were pushing seismic detection shots, and had a Director who was convinced of the value to the country of future nuclear weapons tests. However, Los Alamos had a different kind of advantage. It had a test organization that had been continuously in existence for 10 years, was well organized in its separate aims, and had many years of past data behind it to continue to study. Furthermore, many of the mundane aspects of nuclear weapons testing had been in the hands of Los Alamos up to this time. In 1958, Task Group 7.1, with its some 60 military types to help in test planning, was still based in Los Alamos. EG&G, who could take care of timing and firing and fireball photography, had not learned to work with Livermore and depended upon Los Alamos almost completely for their guidance. While Jim Reeves, the ALOO Test Manager for the NTS, was clearly most respectful of Livermore opinions expressed by Duane Sewell and Gerry Johnson and the Laboratory Director, he was in very close rapport with Al Graves, the LASL Test Division Leader.

Sandia had plenty to do. They could now work on a number of new devices that had been developed for stockpile in the last year and a half. There were new fusing problems which could be solved without nuclear testing. The question of weapons system vulnerability to hostile action needed attention. The development of aerodynamic balloons as possible future test platforms was required.

The Department of Defense Laboratories were, however, in a very bad way, at least until mid-1961, because there was no serious effort to keep their competence alive and because of the reorganization of the Department of Defense mentioned earlier. Air Force Special Weapon Center, for example, went from something like 1000 personnel in 1958 to 14 in 1961. In late 1959 the Secretary of Defense, Thomas Gates, ordered all preparations for nuclear weapon testing stopped as of Jan. 1, 1960.

Within other organizations central to testing, similar efforts were made to extend the life of the capability, although to the participants it may have seemed different. In retrospect, it appears that the lives of the 4950th Air Base Group and the 4926th Squadron in Albuquerque were almost charmed because they had two real-time jobs to do. One, for Sandia they could continue the investigation of drop ballistics of bomb shapes and, hence, had an excuse to continue their bomber capability, now shifting from the B-47 to the B-52. Even more important from the testing point of view, the question of the effluent characteristics from propulsion nuclear reactors being tested in Nevada allowed them to continue a radioactive sampling capability and to exercise that capability occasionally. As is so often the case in this story, the persistence of a radsafe capability through the moratorium was the result of a very few individuals' persistent efforts. The Army Radiological Safety Support Unit (RSSU) had been a mainstay of the rad-safe effort from 1955 to 1958. Task Group 7.1 and the Task Force helped to argue for their continued existence during 1959 and 1960 so they were still available to help in 1961. Much of the JTF-7 rad-safe equipment was transferred to REECo, and was thereby saved. Gordon Jacks was strongly instrumental in preserving this capability, and was again saddled with the job of putting it back together and using it in 1962.

Joint Task Force 7, under Luedecke initially, continued to exist and make plans to conduct future operations. Later under Anderson, as one might expect of any organization that did not have a real-time job to do, JTF-7 began to go downhill. The Navy Task Group, as it had ever since Crossroads, managed in some way to continue its existence, studying the kinds of systems tests which would be of value to the Navy and maintaining, as long as it existed, their coordination with Task Force 7.

Probably the single biggest loss to testing capability in this period was the disappearance of the TG 7.1 J-3 in the summer of 1959. In the previous Pacific operations since Sandstone, this organization had taken the responsibility, between operations, to determine from all the experimenters (AEC, DOD, and otherwise) what their objectives were, what they were trying to accomplish, and what logistic needs they had. All of this was then organized, put on paper, and the appropriate requests for facilities and transportation sent out. Once those facilities were obtained, 7.1 J-3 went into the field and in real time administered that logistic system. The organization for years had consisted of about 60 military people under the guidance of civilian personnel at Los Alamos (with assistance from the other laboratories and AFSWP) and had been headed by such individuals as Colonel Phil Hooper and Colonel Dutch Kerwin (later Vice Chief of Staff, Army). Loss of this organization meant the disappearance of any driving force at a detailed working level to make a continuing coherent operational picture of whatever it was we were planning at the time, and it specifically meant that the technical organizations that would eventually cooperate in performing the future operation no longer had any single point of focus to bring their plans together. This function had not, in general, in the past been carried out by the Task Force headquarters because the Task Force was responsible for carrying out the operation on a large scale and coordinating the efforts of a Navy branch, an Army branch, an Air Force branch, a technical branch, and the AEC branch. JTG 7.1 J-3 was the coordinating spark of the technical branch of the operations.

Clearly, loss of the rest of the JTF-7 organization during the moratorium was also serious, and will be discussed in more detail throughout the rest of this chapter.

The period of the moratorium had many interacting aspects. The rest of this chapter will detail a number of those aspects in approximately chronological order, with the intent of giving the reader a feeling for the number of balls that the jugglers had to keep in the air at one time.

AEC/DOD Actions, Late 1958

As early as October 21, 1958, Starbird had presented to the Commission a proposed weapons test program. Starbird was apparently not convinced that the moratorium would even last the year promised by the President, and in correspondence with the Laboratories was investigating what needed to be done. Eisenhower had stated in his August 1958 messages to the AEC and the Department of Defense that they should maintain their capability to test. It was obvious he was concerned with being caught flat-footed if the Russians were simply playing a game.

There were three main thrusts to the AEC and Laboratory efforts that might maintain a capability to test. One was a readiness program consisting of a continuing series of questions and plans and some minor action over the next three years that kept the subject alive in everyone's mind. The second was the Plowshare program which would clearly use both design and test capabilities. The third had to do with determining the reliability of methods of seismic detection and identification of underground nuclear detonations.

Livermore was, with the cooperation of the Commission, pushing hard on Plowshare. They were studying, among other things, the following items: a channel through the reef at Kapingamarangi; a harbor at Cape Thompson, Alaska; a harbor at Katalla, Alaska; a sea-level canal across the Alaska peninsula at Port Moller; oil recovery from tar sands; an isotope production shot; the creation of artificial aquifers; mining by leaching; and recovery of oil from oil shale. Chairman McCone felt the importance of Plowshare very strongly and had suggested slightly earlier that an AEC committee for Project Plowshare should be established. On November 20, 1958, the Commission authorized the creation of a Plowshare advisory committee. The committee was formed with Spofford English as Chairman and such members as General Doolittle, Bob Wilson, and others. There was clearly hope at that time that the continuing Geneva negotiations could be maneuvered in such a manner as to allow Plowshare shots in the case of a treaty and during the moratorium. At that time, our government was convinced of the value of nuclear explosions for peaceful uses and wanted strongly to continue that effort, and the Russians were not particularly interested, whereas later the situation was reversed.

By December 1958, the question of the validity of the Rainier data, as interpreted for the Conference of Experts, was being reviewed in the light of the Hardtack Phase II data. On December 4 AEC Commission discussion on this subject, specifically between Commissioner Floberg and Paul Foster, noted that the seismic signal was smaller than assumed in the Conference of Experts' report and that the threshold limit for detection for nuclear (underground) tests might be as high as from 5 to 20 kt. An ad hoc panel of seismologists under the auspices of AFTAC met from November 16 to 19, 1958, to consider this question. Carl Romney was chairman and among the members were Frank Press and Perry Byerly with consultants Hans Bethe, Dave Griggs, Ken Street, and Carson Mark. They concluded that it was more difficult to distinguish earthquakes from explosions than had been previously estimated, that the number of earthquakes per year of magnitude equivalent to or greater than a given nuclear yield was about twice that previously estimated, and that, therefore, underground explosions should be carried out to study the effects of explosions in varying geological environments and to evaluate the methods of concealing underground nuclear explosions. Teller felt very strongly that he should announce that underground tests could not be detected under certain conditions, but agreed with McCone not to make any public announcement until the Geneva conference had recessed.

These problems had resulted in appreciable expenditure of effort in the AEC Laboratories. Unfortunately, at that same time, Eisenhower was strongly concerned with the gold outflow and the stability of the dollar in world markets and felt strongly that the federal budget had to be reduced. McCone supported the President in this and imposed the responsibility on the Commission and staff to achieve major AEC budget reductions for FY 1960. As a result, the Plowshare budget was cut by some 25%, and weapons operating funds by approximately 10 million dollars.

Initial Readiness Directions

Several separate but related discussions and studies went into the establishment by DMA of the initial readiness directives at the beginning of the moratorium. The

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first specific guidelines to Jim Reeves came verbally from General Starbird in late September. Reeves communicated these to Al Graves on September 29 as the following:

[redacted]

5 USC 552 (b)(1) Ex. 1 D.O.D.

There had been a meeting in Los Angeles on September 19, of various contractor and AEC test organization personnel, following which H&N had presented cost and task estimates through Calendar Year 1960 to maintain a nine-month response capability to resume full-scale testing of the magnitude of Hardtack Phase I at the EPG.

After some discussion with their Headquarters (Air Research and Development Command), AFSWC, in early October planned to maintain a capability within the 4950th to support a full-scale nuclear test series within six months after cancellation of the moratorium. Further, they planned to maintain development programs to make the best use of data gathered on past tests and continue a vigorous, theoretical laboratory and simulation test program.

On October 12, General Alvin Luedecke, Commander of JTF-7, forwarded for comment to Bill Ogle (JTF-7 Scientific Deputy) a draft report on "Capability for Resumption of Nuclear Testing," which he had sent to the Air Force Chief of Staff for possible forwarding to the JCS and to the Chairman of the AEC. [redacted] Ogle replied to Luedecke on October 17 and, in relation to the readiness response, said:

5 USC 552 (b)(1) Ex. 1 D.O.D.

> It would seem to me that the country can and should maintain the capability to begin testing within three or four months after notification, but I believe no further comment on the number of shots or rate of testing after that time is necessary since it will depend strongly on the information desired from the shots.

Elsewhere, Ogle remarked that both proving grounds should be maintained in a status to accept a moderately rapid buildup.

[redacted]

5 U.S.C. 55[illegible] (b)(1) Ex. 1 D.O.D.

The Chief of AFSWP forwarded the Secretary of Defense Moratorium Guidance (dated September 19) to Field Command and AFSWP on October 26. The Secretary of Defense had stated that the assumption should be that limited test operations might be initiated by February, 1960, but extensive test operations would be started no earlier than mid-1960.

Further discussions on the initial readiness guidelines were significantly affected by the Soviets continuing testing for several days after the 30th of October. Starbird asked both Laboratories to plan the most necessary and fastest response tests that they could do, both overseas and in Nevada. As the pattern typically went, Livermore came up with a number of ideas and pushed for physical preparations to be authorized in Nevada immediately, whereas LASL didn't wish to resume testing for a fair amount of time in order both to reduce the past data and to

[redacted]

prepare for the next tests properly. There were several interesting points in Starbird's hypothetical questions to the Laboratories and within DMA as to alternate readiness positions based on the additional Soviet tests. The discussion began before the beginning of the moratorium (October 28 was the date of the queries to the Laboratories) and included mention of Christmas Island as a possible alternative to EPG overseas and, additionally, the use of Johnston Island. The Commission considered specific nuclear test resumption possibilities in their meetings on November 12 and for the next few days. The possibilities included a specific DMA test readiness paper and a memo from General Starbird to the Chairman of the Commission recommending immediate test resumption if there were additional tests by the Soviets. The Chairman said he was reluctant to seek Presidential approval for additional tests until after the Russians had conducted further tests. However, these discussions did result in swift approval for certain physical readiness preparations at the NTS, including provision to conduct the Adams test. There was discussion of John Foster Dulles's serious concern over further use of Eniwetok because of its status as a U.N. Trust Territory. Hertford informed Starbird on November 13 that reductions at the NTS had put the site in danger of losing its 90-day response capability. He also specified immediate actions in the way of balloon and barge procurement, tunnel construction, and other items to assure the appropriate response.

Thus, out of these initial moratorium readiness guidelines, discussions, and hypothetical propositions, came authorization for a number of physical preparations, as well as specific consideration of alternatives such as Christmas Island and an open sea test capability.

As mentioned before, the AFSWP was in an odd position in December 1958. Personnel of AFSWP knew the DASA charter would be implemented in the spring of 1959. At that time, all military services' nuclear testing budgets would be consolidated into a single DASA budget for FY 1960. That budget would obviously be appreciably larger than the previously planned FY 1960 AFSWP budget. However, there was no certainty that this money would actually be forthcoming because of the uncertain duration of the moratorium and the uncertain need to prepare to resume testing. AFSWP had managed to test (on the Logan event of October 16, 1958) a short section of a vacuum pipe system that was the forerunner of underground effects tests. The results of that small experiment were being studied in order to design a possible follow-on for the circumstance that we would return only to underground testing. Similarly, the Teak and Orange results led to preliminary plans for possible later high altitude detonations.

By January of 1959, most of the test personnel had recovered from Hardtack and the holidays and were back to work to consider again the problems of testing. However, for many months, their efforts were largely devoted to analyzing the results of Hardtack. The operational and construction administrative branches continued the "what if" game for new tests. In January, the TG 7.1 operational staff produced studies on the possibilities of using Johnston, Christmas, Midway, or Eniwetok Islands as bases for a future open seas operation. They also considered the question of quick and dirty balloon shots at Johnston and Christmas Islands.

Evolution of High-Level Attitude Towards Testing, 1958-59

During the first year of the moratorium, just as anticipated by the 1958 Geneva Conference of Experts, the inability to resolve the question of the detectability of nuclear explosions underground and at very high altitude became an obstacle to negotiating a comprehensive test ban treaty. Indeed, the underground detection and identification problem became more and more difficult because of additional data from the Hardtack II tests. The data, as interpreted by an AFTAC Panel and the "Berkner" Panel, indicated poorer sensitivity than earlier indicated by the Conference of Experts. The Latter big-hole theory (potential of decoupling in a large cavity) and the lack of experimental data on decoupling in various media under various conditions were indicative of the need for more research on underground explosion detection and identification. In the first half of 1959, the Panofsky Panel on High-Altitude Detection made very clear that outer space test evasion methods were feasible, and methods of detection of such tests should be studied. Those who were most earnest in their desire to have adequate verification of a full test ban were more pessimistic about adequate detection in underground and outer space environments in the spring of 1959 than they had been after the Conference of Experts.

The AEC shifted their emphasis in treaty negotiation discussions (which affected test readiness activities) to consideration of the concept that the test ban should be reached in phases, with any initial formal agreement excluding the underground and outer space environments for the time being. Through 1959, this consideration was increasingly coupled with the fear that an ongoing, unpoliced, complete moratorium allowed other nations to conduct clandestine tests in the underground and outer space environments without detection by the currently installed systems.

President Eisenhower was surprised at the adoption of specific, agreed, control guidelines at the Geneva Conference of Experts, where he had expected the Soviets to take more of a political stand. However, he found the Soviets far less willing to adopt the Conference of Experts' system when the political negotiations began after October 30, 1958. Noting the veto that they seemed to require on the Control Commission and their demand that each nation's control posts be staffed by government nationals, he felt this "made it obvious they had no intention of agreeing to a practicable control system."* This sort of feeling, coupled with the new underground test data which further confused the issue of adequate control measures, led Eisenhower to propose to Khrushchev on April 20, 1959, that a limited nuclear test ban only for the atmosphere be addressed at the conference.

The community attitude towards testing and test readiness in this time period was exemplified by the guidelines given to, and reports of, a committee addressing future test methods and testing organizations, chartered jointly by DASA, JTF-7, and DMA. Their report reflected the guidance that testing in the atmosphere, as opposed to underground and in outer space, was highly unlikely.

Thus, by April of 1959, without explicitly coordinating their positions ahead of time, the AEC, DOD, Department of State, and the White House all came to focus on underground and outer space testing as the likely future techniques. Perhaps more importantly, they tended to treat the old style of testing in all environments as so improbable as to receive little attention and less funding. These philosophies were solidified in discussions between the President's Scientific Advisor (Killian), the Deputy Secretary of Defense (Quarles), and Chairman McCone in late April 1959. This reasoning significantly warped test readiness considerations for the rest of the moratorium period.

In light of these positions being fairly well defined in the minds of many people by the summer of 1959, and not forgetting Dulles's position that the EPG was not a desirable area, owing to its trust territory status, the decisions to downgrade the status of that proving ground, from August of 1959 on, are not a surprise. The so-called "minimum maintenance" status to be retained at that location, following the inactivation of the Army support unit there (Joint Task Group 7.2), was to keep the

*Dwight D. Eisenhower, *Waging Peace, 1956-1961: The White House Years*, Doubleday, New York, 1965, page 478.

U.S. ready to resume full-scale testing as in the past or within a year of authorization. This was felt to be more than adequate based on the improbability of resuming that method of testing.

The experts' conference at Geneva on high-altitude detection, known as Technical Working Group I, concluded several weeks of joint discussions on July 10, 1959, with an agreement proposing the establishment of a system of both earth satellites and ground control posts to detect high-altitude detonations. Perhaps this encouraged many of the decision makers, particularly the nontechnical personnel, to suppose that the technical details of adequate enforcement in all environments was possible, and rekindled a hope that with a similar conference on the underground environment, the negotiations might again be on a track leading to a comprehensive agreement.

The Russians, with reluctance, did agree to a look at the new U.S. theories and experimental data as part of a technical experts' conference on the detection and identification of underground nuclear detonations. This became Technical Working Group II (TWG II), which met at Geneva in November and December of 1959. Various observers felt that this technical conference, in contrast to previous joint conferences, was not totally objective but had strong political overtones. It did not result in agreement on needed research, development, or treaty requirements. In fact, each of the three delegations submitted separate reports with the U.S. and U.K. agreeing in substance, and the Soviets disagreeing on almost every technical conclusion and recommendation of the U.S. experts. By the time TWG II met the Soviets had taken an ambiguous stand on the issue that the U.S. felt was the key to enforcing a comprehensive test ban in the underground environment, that of on-site inspections. On July 9 the Soviets agreed to the principle of an annual quota of inspections, but would not agree to a definite number. Thus, the U.S. remained optimistic, through the last half of 1959, that the Soviets might agree eventually to safeguards that the U.S. felt were sufficient technically. On the other hand, anxiety grew that the Soviets were "stalling" while they cheated or prepared to cheat. The lack of agreement at TWG II made the U.S. even more pessimistic about a comprehensive treaty.

In August of 1959 Eisenhower extended the one-year moratorium to the end of the year to permit more time for negotiations and technical discussions. Later, noting the Soviets' unwillingness to consider all of the technical data in reaching a satisfactory agreement, he allowed the moratorium to expire on December 31. He considered the U.S. free to resume testing, but pledged that we would not do so without announcing our intentions in advance. He also stated that during this period of voluntary suspension, the U.S. would continue an active program of weapons research, development, and laboratory experimentation.

The Commission position, and the personal position of Chairman McCone, was clarified by a number of public statements and Commission discussions through 1959. Perhaps to lay the groundwork for policy decisions on test readiness authorization, Chairman McCone stated, at a meeting of the principals (Secretary of State, Secretary of Defense, etc.) on October 6, that the Commission felt the Geneva negotiations toward an agreement should continue, but he proposed that if the negotiations did not reach an agreement, the U.S. announce a unilateral moratorium on atmospheric tests. With respect to underground tests, he proposed to reserve the right to take action as deemed necessary, perhaps after the first of 1960.

Air Force guidance*, circa April 1959, was "No actions are to be consummated which would jeopardize or reduce the Air Force capability for continuous development and subsequent testing immediately following the termination of any test moratorium.

*Hdqts. 4950th Test Group, Readiness Report, 1 April 1959.

Special care must be applied to prevent any degradation of capability in areas of planning, programming, personnel and other supporting activities."

On July 9, 1959, at a meeting of the Executive Council of the French Community, France announced their intentions to conduct nuclear tests in the Sahara Desert. The first explosion was not conducted until February of 1960. The entry of France into the nuclear weapon community exacerbated the problems of obtaining international agreement to a CTB.

At a Commission meeting on December 11 Chairman McCone noted that the Joint Committee on Atomic Energy "fully supports the Commission's position on the importance of adequate safeguard positions in any test ban agreement with the Soviet Union." Senator Anderson urged that the Commission be in the position to test a number of devices immediately after the first of the year or as soon as the test moratorium ended.

Seismic Detection/Latter Hole, Early 1959

In early January, the Commission turned its attention to the question of providing guidance for U.S. negotiators at Geneva concerning seismic detection. The United States officially released its new data suggesting that the Geneva experts' system would have a threshold closer to 20-kt than to 5-kt. The Commission expressed its concern that it would take six to eight months to conduct further underground tests to determine the seismic detection threshold. Another complication came in January when Dr. Albert Latter of Rand Corporation, apparently at the suggestion of Edward Teller, announced a decoupling theory by which a shot could be fired and produce only about 1/300 of the seismic signal that had been previously assumed. This decoupling phenomenon was effected by firing the shot in a very large spherical cavity, the required diameter of which could be calculated in accordance with the theory. However, there was appreciable concern about the validity of this theory at the time and for many years afterwards. Nevertheless, it was accepted by the PSAC, including Hans Bethe and Edward Teller, in late January. Since now it appeared that a large shot could be fired without detection if the evading country were willing to build such a cavity, the introduction of this theory, as probably intended, led to confusion as to the guidance to be given to our Geneva negotiators.

Laboratory Attitudes, Early 1959

During early 1959, the Laboratories and DMA continued to plan nuclear weapons tests, at first considering times only a few months away, but later settling onto November 1959 as a possible date, since that was when the one-year moratorium would run out. Livermore tended to concentrate its thinking on renewed underground testing and pressed for more tunnel construction in Nevada. Los Alamos was more concerned with overseas atmospheric testing, including high-altitude shots, while Sandia prepared for any of these concepts. The pressure from the Commission was toward the conduct of underground tests for seismic detection research, with a concomitant drop in interest in nuclear weapons tests. Los Alamos was strenuously against underground and outer space testing. In March, Al Graves took the opportunity to make these feelings known to the General Advisory Committee's Weapons Subcommittee during their meeting at Los Alamos, intending to counteract some of the Livermore enthusiasm for underground testing given to the JCAE the previous July at Livermore.

Weakening of JTF-7, Early 1959

In March 1959, less than five months after the beginning of the moratorium, questions arose concerning the function of Task Force 7, its organizational structure, and even its continued existence. This question was apparently first raised by the Deputy Secretary of Defense, Don Quarles, in a March 7 letter to Mr. McCone. The question arose because of the ongoing reorganization of the Department of Defense, specifically the planning for greater consolidation of atomic weapons activity in AFSWP. The new thought was to effect greater economy by transferring the functions of the Task Force to DASA. In April, a study group consisting of General Starbird (DMA), Admiral Parker (AFSWP), and General Anderson (JTF-7) was set up to study this question. In addition to the question of money and the reorganization of the Department of Defense, it was recognized that future tests, especially effects tests, might require more coordination between the AEC and the Department of Defense than in the past. Loper asked General Anderson to consider this point. Somewhat optimistically, it was suggested that future AEC shots could be prepared and held until the effects community was ready, rather than being tied to previously specified operational dates.

It is interesting to note the judgment of this study group on the likelihood of different methods of future testing. They felt that contained underground testing was most likely to be permitted and that a limited amount of preparation should be conducted so that such testing could proceed with minimum delay. Tests at altitudes greater than 50 kilometers, which was the presumed limit of detectability of the Geneva experts' system, were considered somewhat less probable, so that no preparation should be made for such tests, but investigations and plans for this type of testing should be kept under continuous review. Atmospheric tests at the NTS were considered of such small probability that no detailed planning or preparation was warranted. Atmospheric tests in the Pacific at sites other than Eniwetok or Bikini should be studied, but no appreciable expenditure of funds should be allowed. The likelihood of atmospheric tests at Eniwetok Proving Ground was considered so small that the Proving Ground should be put on standby status and only those expenditures should be made that would prevent significant deterioration of essential facilities. It was assumed that test preparation could begin nine to twelve months before extensive firing was required. Johnston Island had already been returned to Air Force command, and plans were being made to transfer it to the Army on or about January 1, 1960, for launching missiles for the Nike-Zeus program. The group noted that Johnston Island would be in an operational status within 18 months, which was also the stated readiness time to do Operation Willow, and suggested that the Army could support nuclear tests launched from Johnston Island using the same people who would already be there for support of the Nike-Zeus program. Thus, a few people, mainly a test director and staff, could go to Johnston for a short period of time to conduct any high-altitude tests. They therefore recommended that future agreements between the services should be made with this concept in mind.

The study group recommended that JTF-7 become a subordinate command to DASA, with liaison with the AEC maintained through DMA. The AEC would, of course, acquire joint control after the decision had been made to resume testing. Specifically, it was recommended that Task Group 7.1 (the scientific group) be disestablished because it would obviously not be required during the interim period. That disestablishment was to be completed by August 31, 1959.

It is interesting to speculate why this particular recommendation was made. None of the three gentlemen making the decision had had task force experience, although Starbird had had a great amount of contact with Luedecke during the 1958 series. Al Graves had offered some resistance to the decision to discontinue Task

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Group 7.1, but he was in Geneva at the time and couldn't argue very effectively. There was very little support for its continuation from the AEC laboratories (other than LASL) or from the DASA testing organization. On June 5, 1959, Don Shuster resigned as Task Group Commander, and on June 25, 1959, General Order No. 5 ordered the discontinuance of Task Group 7.1 effective August 31, 1959. Thus, the organization that had conducted the technical, operational, and logistics work for overseas tests for over 10 years was disbanded.* In retrospect, this move appears as possibly the most serious single move made during the moratorium toward winding down our capability to test in the atmosphere. In overseas operations prior to 1959, the senior laboratory representatives in the field, commonly heading Task Units of 7.1, thought their major responsibility was to their home laboratories, or, in the case of DOD, to Field Command. The JTG 7.1 staff was the next layer up in the field and planning organization and, therefore, was still extremely sensitive to the needs of the basic experimenters in accomplishing their work. The Task Force staff, however, and to a certain extent the other task groups, were isolated by the very nature of operations from detailed knowledge of what those requirements were, and hence tended to look upon the experimenters as an unreasonable group of people simply trying to feather their own nests and make life hard for everyone else. In the author's opinion, the result was that after August of 1959, the Task Force lost touch with reality, at least as far as the AEC requirements were concerned, because it no longer had any channel at all to accept the thinking of the AEC laboratories.

Overall, the Loper study group recommended an 80% reduction in currently authorized spaces, with Task Force Headquarters going to 77 people, the Army to 32, the Navy to 90, the Air Force to 7, and of course, the scientific task group to 0.

In retrospect, it is difficult to see what these gentlemen were up to: by the time the report was finished, the concept of future operations had been reduced by others to that of underground tests in Nevada (for which a Task Force wasn't needed), and deep space tests launched from Johnston Island, with a remote possibility of large-yield atmospheric shots fired in some not-quite-clear way, perhaps by a floating task force in the Pacific. [redacted] So it's clear that Task Force Headquarters, left at 77 people, was barely needed. 5USC55. (b)(1) Ex. 1 D.O.D.

JTG-7.2, the Army, reduced to 32, at least had a job of maintaining housekeeping at Eniwetok, although that could easily be done by the AEC alone since Eniwetok reverted to them, in principle, in non-operational phases.

The Navy task group, left at 90, was needed mainly for planning Navy systems tests, whereas the Air Force, which was essential to almost any concept of operations in the atmosphere, either AEC or DOD tests, was reduced to seven. And the technical task group, 7.1, required for any technical planning at all, was reduced to zero.

Amazing!

Treaty Progress, Early 1959

At Geneva the arguments continued, largely about the number of inspection stations which had now been confused by the seismic detection uncertainties mentioned

*The going-away party used up all but $200 of a fairly large sum collected for the recreational fund (7.1 rec. fund) over those 10 years.

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before. In fact, Eisenhower in April proposed to Khrushchev an alternative by which the test ban should be put into effect in phases, starting with the prohibition of nuclear weapon tests in the atmosphere up to 50 kilometers, while the negotiators went on to seek methods of extending an effective test ban to underground and outer space. Khrushchev rejected that proposal and went back to the argument about on-site inspections.

At Livermore, Teller apparently felt there was a high possibility of testing being allowed in the very near future underground or in deep space, simply because there was no way to come to a satisfactory treaty agreement on those subjects. However, Los Alamos seemed to feel it was being driven to underground tests against its will. George Cowan commented in March 1959 that all the experience in underground radiochemistry was at Livermore (LASL's two previous underground shots had come out of the ground so nicely that samples were obtained above ground). He also commented that if we had to test that way, then he supposed we would learn how. To paraphrase LASL views: "Either weapons testing is worthwhile or it isn't. If it is, let's do it properly; and if it isn't, then let's have a treaty." Livermore, sparked by Teller and Harold Brown, was doing everything it could to move toward a treaty that would still allow testing. It's interesting that at this point, Harold Brown, after returning from Geneva, proposed a treaty, apparently based upon observation of nuclear testing by the use of satellites, that would not involve either a threshold limit or inspection teams.

Test Planning, First Half, 1959

Starbird continually requested that the Laboratories update test plans, but he also did his best to accomplish, in this hard time, all of the real work necessary to maintain our nuclear weapons capability. Thus he went as far as he could to cooperate with the Laboratories' Directors in a manner to best benefit their internal programs.

At the Nevada Test Site, as a result of LASL's insistence upon firing in vertical holes in Area 3, the ugly problem of groundwater contamination was raised. Some shots had to be fired below the water table, and hence, assurance was necessary that the contamination would not be tranported by underground water to some embarrassing spot off-site or to producing water wells. Further studies were initiated to investigate the ground-water problem.

The Laboratories and AFSWP, in the light of the new testing philosophies, were seeking facilities for NTS underground tests. LASL, in the person of Bob Newman, requested in April that ALOO (Reeves) develop a plan for the construction of four tunnel sites on Rainier Mesa (estimated to cost 7 million dollars) and four 1100-feet holes in Area 3 (estimated to cost 3 million dollars). At the same time, Ken Street of Livermore suggested to DMA that maintaining a 90-day readiness would result in extensive wasted effort and funds, and suggested instead preparation for continual underground nuclear weapon testing to be started 12 to 18 months later.

[redacted] Also included were some holes in granite and

5 U.S.C. (b)(Ex. 3 D.O.D

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marble for the seismic detection program. One of the Livermore tunnel sites was for the DOD shot, Jericho, later renamed Marshmallow. The tentative construction budget to finish this work was 52 million dollars, of which about half was available. Several of the Livermore tunnel sites had already been constructed or were funded, whereas only the LASL safety shot holes were in that situation (four 500-foot, 36-inch diameter holes). Diagnostics in the proposed underground series would be minimal. [redacted]

5 USC 552 (b)(3) Ex. 3 D.O.E.

Deep Space Testing, May 1959

By now (May 1959), the system seems to have convinced itself that the only "atmospheric" testing that would be allowed and, hence, was worth planning for, would be conducted at altitudes above the Geneva system detection limit of 50 kilometers. The Berkner Panel (who met in early 1959 and published their results on March 31, 1959) had suggested looking at the problems of testing at altitudes above the limit of detection by the Geneva system. McCone had met on April 23, 1959, with Killian, Quarles, and Starbird and agreed the AEC would look into this question. Livermore felt that such shots could probably be performed in about 12 months, obtaining only very rough diagnostic data, and that in about 18 months fairly acceptable measurement techniques could be developed. At the previously mentioned meeting in early May (which included Starbird, Bradbury, Jane Hall, Ogle, Mark, Teller, Johnson, Foster, Herbst, Fowler, Shuster, Hertford, Reeves, Loper, Parker, and others), the high altitude program was discussed. [redacted] Each device would be lifted on a modified Redstone from Johnston Island to 500-1,000 km altitude. The earliest launch date would be August 1960. The DOD was already planning on the Willow program of six tests, four utilizing the Redstone missile, one the Jupiter missile, and one a balloon. It was also agreed that the possibility of performing the AEC tests at altitudes of 100,000 kilometers or more would be investigated, as suggested by the Panofsky Committee of the President's Science Advisory Committee. Such tests would require four different boosters (Centaur, Atlas, Vega, and Saturn), with readiness dates ranging from late 1960 to 1965 (see Table V). The estimated costs for the lower-altitude AEC program were 185 million dollars, whereas the system to test above 100,000 kilometers would cost some 320 million dollars. Sandia was already looking into the problems of missile failure, destruct systems, and associated safety devices. Problems of retinal burn, electromagnetic interference, and atmospheric fallout were recognized.

5 USsC 55 (b)(3) Ex. 3 D.O.E.

[redacted] 5 U.S.C. 552 (b)(3) Exemption 3, D.O.E.

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TABLE V
A. PROPOSED AEC HIGH-ALTITUDE PROGRAM
May 7, 1959

[redacted]

[a]Diagnostic package weight for all devices would be 600-700 lb.

[b]For altitudes of 500-1,000 km the carrier would be the modified Redstone; ready date, August 1960.

NOTE: Use of the improved Atlas would allow testing the 3000 lb warhead at 25,000 km and all of the others shown at 50,000 km commencing in mid-1961.

B. DOD WILLOW HIGH-ALTITUDE EFFECTS PROGRAM

[redacted]

5 U.S.C. 552
(b)(3)
Ex. 3, D.O.E

Clandestine Test Detection, Mid-1959

The forerunner of what was eventually to become the Vela Program was now beginning to solidify. At the previously mentioned meeting of McCone, Killian, Quarles, and Starbird in April 1959, the AEC agreed to cooperate in following the Berkner Panel recommendations relative to undertaking an experimental test program to determine the parameters of detection and concealment. Within the AEC, Livermore, in conjunction with Rand, had considered the underground detection question more than others so that that work fairly naturally fell to them. During discussions of the possibility of combining proposed weapons tests underground with proposed seismic detection shots, LASL did express an interest in the seismic detection program, but pointed out in late April that it was inconsistent to combine these two aims since a test shot by definition might not give the predicted yield. LASL, because of its experience and interest in atmospheric testing, somewhat naturally took on the problems of high-altitude detonation detection. Using ground stations, they would observe the fluorescent light and electromagnetic signals resulting from the detonation. In conjunction with Sandia, Los Alamos began to look seriously at the question of detecting deep space shots using instruments in satellites. This work was, of course, in conjunction and in cooperation with ARPA and AFTAC, who had the basic responsibility. To further this work a joint Los Alamos-Sandia working group (called the Buzzer Committee) was set up with Dick Taschek of Los Alamos as the Chairman. Membership was drawn from the physics, test, and theoretical divisions at LASL, and from Sandia. It was expected that it would take this committee four to six months to come up with anything significant beyond the recommendation of the Panofsky Panel Report. Consequently, the Laboratories suggested that the Panofsky report was adequate to provide any interim need of the Geneva Panel of Experts or others.

In mid-May, Starbird, worrying about information for the upcoming working group meetings in Geneva, asked Livermore to consider the necessary programs and time scale to obtain acoustic data from extremely small detonations in blocks of salt or other material. Later on, high-explosive detonations in the appropriate media would be conducted in a further effort to try to understand the observation during Hardtack of very different seismic signals from two tests at essentially the same yield (Tamalpais and Evans).

[redacted]

5USC552 (b)(3) Ex. 3 D.O.E.

Starbird also requested that Livermore and the operations offices investigate the feasibility of testing the Latter hole theory using high-explosive detonations in the salt mines of Louisiana and Texas. He informed the Commission that two such salt mines had been identified. These tests could be conducted in something like 60 to 90 days.

Effects of Moratorium

In early 1959, the system began to realize the penalties of not testing. At the early May meeting previously mentioned, the representatives of DMA, DASA, ALOO, Laboratory Directors, and the Military Liaison Committee concluded that: (a) Foregoing all testing in the future would limit the warheads that the AEC could offer to the DOD to meet existing or near-future system requirements (certain warheads offered by AEC would fall short of DOD desires in regard to assurance of performance, amount and predictability of yield, or other characteristics); (b) the exploitation of certain fields of longer-range DOD interest which could lead to significant changes in weapons systems and doctrine could not be accomplished without further testing; (c) foregoing further testing would preclude obtaining effects information required by the DOD, of which high-altitude effects were the highest priority.

Sandia Balloons, 1959

In spite of the very strong opinion that only detonations underground or at very high altitude would be allowed in any future tests, assuming a complete test ban was not agreed to, some work continued along other lines. Sandia, who had designed and operated the tethered balloons used in earlier operations in Nevada for lifting nuclear devices for detonation, now began the design and development of a balloon capability for Eniwetok. The concept was developed by Don Shuster, as Commander of 7.1, and others in the Task Group and at Sandia. The AEC, through NVOO and Sandia, supported the development. By June of 1959, Sandia had had test flights at the Nevada Test Site of balloons carrying up to 20,000 pounds of payload, indicating that it would be feasible to lift such weights to altitudes of some 5,000 feet at the Eniwetok Proving Grounds. Coaxial cable for balloon use at NTS was being procured in July of 1959, and further prototype testing of a 20,000-pound lift balloon with a 15,000-pound payload was planned for September and October. Shuster reported in December that aerodynamic balloon operations could proceed about 10-12 months after authorization (5 months for test balloon delivery + 2 months for test flights + 3 to 5 months for production unit delivery).

Reduction of EPG Capability, 1959

During the earlier Pacific Operations, the Army Task Group had been responsible for "housekeeping" at the Eniwetok Proving Grounds. They furnished the Island Commander and his staff, many of the military vehicles required, airfield operation and maintenance at Eniwetok Island, and many other such functions. During the early operations, the Army had been most willing to perform this function as one of the ways of getting into and staying in the nuclear weapons business. However, the Army portion of the nuclear weapon pie gradually reduced in the later part of the 1950s and the manpower drain to continue this function was appreciable (the Army had 1,000 personnel at Eniwetok at the end of 1958). At the end of Hardtack Phase I, the TG 7.2 (Army) Commander, Colonel Stanley Sawacki, recommended to CJTF7 that the Air Force assume responsibility for all the military functions at Eniwetok presently assigned to TG 7.2, that the AEC contractor (H&N) take over the other TG 7.2 functions, and that TG 7.2 be inactivated. Initially, this proposal received no particular attention in the light of the uncertain future of JTF-7 itself, as mentioned earlier. However, later, against the background of the general set of studies and moves going on in mid-1959 (i.e., the Department of Defense reorganization, the agreement to put JTF-7 under AFSWP, and the reorganization of AFSWP into DASA), the suggestion was looked upon with favor. The requirement that the Army support the Nike-Zeus test program on Johnston Island made the relief from maintaining Eniwetok even more welcome. As a result, Task Group TG 7.2 was reduced to less than 400 people at Eniwetok by the end of July 1959.

The "Report of the Study Group on Organization for Future Test Operations" was endorsed by CJTF-7 (Anderson); Chief, DASA (Parker); and Director, DMA (Starbird), on August 20, 1959, and sent to Secretary of Defense, the AEC, and the JCS Chairman. The report recommended, among other things, that at the Eniwetok Proving Ground there be a general move to standby status by such actions as consolidating all base camp activities to Eniwetok Island with mothballing of the facilities on Parry, which had been the headquarters of the technical organizations, reductions in the size of the boat pool, elimination of locally based aircraft, and no further construction. Total personnel were reduced to 495. Anticipating the study completion was the official notification of a standby status phase-down to TG 7.2 and others on August 8. By

September 1959, Operation Switch was taking place with TG 7.2 handing over its responsibilities and equipment to Holmes & Narver. By December of 1959, TG 7.2 was down to 20 people on Eniwetok. This study group report, which came out just two days less than a year after the President had announced the test ban moratorium, and the concomitant actions resulted in the conversion of an active and useful proving ground to an almost useless piece of real estate in a period of about a year and a half, mainly on the assumption that testing in the atmosphere would not be allowed in the future, even if we were to return to testing.

It appears that not a single person in the whole chain of decision makers at that time, all the way to the President through PSAC, the Commission, the Department of Defense, and the Laboratory Directors, actually believed that there was any serious hazard associated with worldwide fallout that might be produced by any future nuclear testing in the atmosphere. Rather, there was judgment that if negotiations were to break down because of the inability to solve the underground detection and identification problem, reaction to public fear of fallout would result in at least an atmospheric test ban. The reduction of total test funding, both because of the need to reduce the total national budget and the conviction that we would not have to go back to testing at all, contributed strongly to the degradation of the Eniwetok Proving Ground. In retrospect, however, the author believes that had the Eniwetok Proving Ground been maintained it would have been used in 1961 and 1962. Most of the development shots would probably have been balloon lifted rather than airdrops. Also in retrospect, however, it's not a bit clear that any more weapons development information would have been gained that way than actually was obtained at Christmas Island, nor is it clear that the operation would have actually been conducted any sooner.

Plowshare, Early 1959

Livermore had long been promoting the peaceful uses of nuclear explosions (Plowshare). Clearly, the pursuit of this effort in a period of no weapons testing could be helpful in maintaining a weapons testing capability. The device designs were similar, but Plowshare devices did not have to meet the rigid strength and size criteria required of weapons. In addition, the criteria on cleanliness might be different. The testing of a device to be used for Plowshare purposes used essentially the same observational techniques as those for a weapon test. Many of the experiments could be performed with devices designed to be used as weapons. Edward Teller and Gerry Johnson of Livermore pressed to separate the peaceful uses program from the weapons test problem, urging that any test ban allow the continued use of nuclear explosions for peaceful purposes. They urged that arrangements be made for Plowshare experimental detonations in the then extant circumstances of a weapons test moratorium. The Russians were not enthused about the Plowshare concept, and pointed out that it would be very difficult to differentiate a Plowshare explosion from a weapons test explosion. It was clear in the AEC family that such a differentiation would be most difficult; and, in fact, it was clear that unless there was extremely detailed monitoring, it would be very simple to conduct weapons tests under the guise of Plowshare. This latter politically difficult point led to a sort of schizophrenia in the community, in which it was simply not proper to admit the possibility of using Plowshare for evasion purposes. Hence, the Plowshare discussions were usually kept separate from the weapons discussions. This same feeling led a little later to the Plowshare program being separated within the AEC Headquarters from the weapons development program, resulting in a separately labeled budget for planning Plowshare detonations.

During January of 1959, preparations continued. EG&G was constructing alpha

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measuring equipment and a portable timing and firing system. Livermore was making detailed plans, while Los Alamos re-examined the value of participating in the Plowshare program. Previously LASL had not been particularly interested in Plowshare as such, feeling they were already overloaded with weapons problems. However, under project SANE (Scientific Applications of Nuclear Explosions) work had been done on the possibility of producing and recovering large amounts of transplutonic elements by means of underground nuclear explosions. (A number of the transplutonic elements had been produced in several previous shots, including Mike, but the devices had not been designed to maximize such production.) Production of electrical energy by underground nuclear explosions was also considered.

Attempts were being made early in the year to formulate proposed international rules by which Plowshare shots might be conducted. Early announcement to other nations giving the date, the place, the purpose, the yield, measures to minimize fallout, etc., was suggested.

By mid-1959, physical preparations were being made for two Plowshare demonstrations. One was Project Chariot, to produce a harbor at Cape Thompson in Alaska, to assist in the development of the region.* Chariot consisted of a cratering shot of 100 kt yield at about 700 foot depth, to produce the harbor, and an additional four 20 kt shots to produce a channel connecting the harbor to the ocean. Environmental studies of the region, including engineering considerations, were being conducted at this time. Project Gnome, a 10-kt shot to be fired in a salt dome in southern New Mexico, was planned to study energy and isotope production. In mid-May, the Commission approved expanded effort on these projects. The Plowshare group at Livermore was, by this time, of appreciable size. A number of the group members also were part of the weapons test organization.

An example of the feedback of Plowshare considerations to weapons test capabilities is shown in a message about this time from Ed Fleming of Livermore to Colonel Thompson of AFSWC concerning future air sampling capability. He offered his opinion that for a long time to come, only cratering-type Plowshare shots would produce radioactive clouds, that these could probably be sampled by the drone aircraft sampling system then being developed by Sandia and that, therefore, the efforts of the 4926th Sampling Squadron would not be required after the end of 1959.

NTS 90-Day Readiness, Late 1959

By mid-June 1959, the testing system had developed reactions to the guidance offered from the May 7 meeting of DMA, Lab Directors, DASA, etc., previously mentioned. LASL, after due consideration, went back to its old stand that it preferred vertical holes to tunnels and formally requested that ALOO design and construct four 1,100-foot holes of 36-inch diameter in Area 3, and cancelled their request for tunnel work at Rainier Mesa. Discussions at the June 4, 1959, Nevada Planning Board meeting centered around the "requirement" to meet a 90-day readiness for underground testing. It was concluded that such readiness could be achieved by November 1, 1959, if prompt approval were given for the LASL 1,100-foot holes and for the desired LASL mobile alpha station. At that time the laboratories would be ready to do approximately one shot each per month, assuming continued drilling and tunneling were

*Envisaged at that time was, strangely enough, the use of the harbor as the end point of an oil pipeline from what has since become known as the Prudhoe oil field, allowing shipping most of the year out of that harbor to the lower 48. The pipeline would be almost continuously over hardrock and would therefore not face most of the environmental difficulties that the present pipeline faced.

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approved. A 550 $W^{1/3}$(ft)* rule for detonation depth was accepted in spite of the containment difficulties with tunnels during Hardtack. LASL indicated that they would be happy to have some release of activity to improve the possibilities of sampling for radioactive debris, whereas Livermore intended complete containment. (Livermore had started an attack on the concept of prompt sampling through small pipes from the shot point during Hardtack.) Sandia was also preparing their balloon lift capability to be ready on a 90-day notice for shots in two or three areas at NTS. LASL initiated effort on the design and field check of a method for cleaning the contamination from all the reusable vertical holes used in Plumbbob and Hardtack. It was estimated that this action might make four 330-foot, 36-inch diameter holes available for one-point detonations. The 1,100-foot vertical holes requested by LASL would cost some 1.9 million dollars, which had not yet been approved. Construction had been authorized and was under way for three safety shot sites in Tunnels I, J, and K for Livermore. However, authorization to prepare the full-yield shot sites was still required. Preparation was estimated to cost some 3.6 million dollars in FY 1959, 5.5 in FY 1960, and an additional 9 million once the go ahead for actual testing had been received. In mid-June, Gerry Johnson of Livermore requested authorization from DMA (Starbird) to begin the new construction necessary if readiness were to be achieved by November 1. On June 22, Starbird withheld such authorization pending further review. At the midyear review on June 25, 1959, Norris Bradbury (LASL) emphasized that LASL intended to make its decisions and conduct its programs such that they could be abruptly modified as the future course of testing became apparent. He felt it unlikely that the U.S. would again test in the lower atmosphere to any extent and commented that the probability of resuming nuclear testing under any circumstances was about 50%. He commented that LASL did not plan to devote any appreciable effort to the problem of elaborate physical diagnostics underground until it was clear that there actually would be a test series. He further commented that LASL intended to make extensive use of the capabilities of Sandia to assist in diagnostic measurements of exoatmospheric detonations should testing of this type be undertaken. He also commented on his intent to shift about 10 percent of the current LASL testing personnel to other programs in the next year, assuming there were no extraordinary changes in the testing scene.

As of mid-1959, Starbird had requested that the initial January AEC testing budget of 17.5 million dollars for full-scale tests be upped to 27.5 million for FY 1960. This upward revision included the cost of doing preparatory work for possible underground testing in Nevada. It was his assumption that of the 27.5 million, some 8 or 9 million would go into minimum maintenance effort at the Eniwetok Proving Ground. A little would be reserved in case some method of testing other than underground should become possible, and about four million dollars would be used for continued tunneling in preparation for possible full-scale weapons tests (approximately one million dollars of that was for the Jericho shot). This money would support some 150 miners engaged in tunneling, but he suggested that the number be dropped to perhaps 100 by July 1, 1959. Approximately two million dollars would be spent on construction for shots connected with the underground seismic program and about one and a half million on various efforts concerned with the problem of water contamination at the Nevada Proving Ground. To allow flexibility as the situation became clearer in the latter half of 1959, 4.8 million would be left uncommitted.

By July 1959, LASL, in conjunction with Reynolds Electrical and Engineering Company and NVOO, was well into the design of the operational and mechanical

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*The product of the number 550 and the cube root of the yield--for yield expressed in kilotons--is to be the depth of burial in feet; i.e., for 1 kt, the depth would be 550 ft; for 1,000 kt, depth would be 5,500 ft.

procedures for firing in their proposed 1,100-foot holes, and the AEC had given approval to procure some 20,000 feet of coaxial cable for use with possible balloon shots at NTS. Design was under way for a downhole catcher to collect radiochemical samples which could then be pulled up through the sand of the backfill.

As a result of the uncertainty concerning groundwater contamination in Nevada, Reeves contracted with the USGS (Bill Twenhofel) to begin an investigation of this problem at NTS. A 1,200-foot hole was drilled in the north-central part of the Tippipah Springs Quadrangle during July and August 1959 as a beginning of a ground water monitoring program.

In mid-July, LASL changed its request for deep holes from 1,100 feet to 1,200 feet in order to make the hole depth good for 10 kt. ALOO had prepared the advance notice for bids for these four deep holes, but was holding them pending DMA authorization of construction. LASL was having difficulty completing their experimental design for these holes because of the problem of getting the boost region alpha signal up the size cable they felt was reasonable to put down 36-inch diameter holes. By August 6, some five different downhole canister geometries with various diagnostic capabilities had been proposed.

All three weapons laboratories were working hard on the problem of containing radioactive debris underground. LASL was, for a change, taking the subject seriously, and in late August, J-15 published a set of computations predicting the proper depth of burial. However, there was no change in the officially approved $550W^{1/3}$ criterion chosen by the planning board in early June.

While work was continued on the tunnel complexes for LRL and the Department of Defense, approval from Washington did not come for the LASL 1,200-foot holes. Thus, toward the end of September, LASL returned most of its equipment to New Mexico. An internal LASL report of the period includes the comment "the enormous quantity of work that Holmes & Narver has had to do for LRL weapons, Vortex and Plowshare, has made H&N progress on LASL designs very slow. There is small indication that they will divert more effort to our projects any time in the near future."

On August 26, 1959, President Eisenhower announced that the United States would extend its unilateral testing suspension to the end of 1959. On the 27th, the United Kingdom stated that it would not resume tests as long as the negotiations showed prospect of success, and on August 28th, the U.S.S.R. pledged not to resume testing unless the western powers did so. This obviously did away with the concept of beginning an underground operation on November 1, 1959, but for a little while, the planning went ahead with the same concepts as those expressed in May, but delaying the time at which testing might resume.

Livermore continued to refine their plans. On October 19, 1959, Myron Knapp outlined in an internal document a plan (Succotash) for reaching a readiness-to-test capability for LRL [redacted]

5USC (b) Ex. D.O.

[redacted]

5 U.S.C. 552(b)(3), Ex. 3, D

5 U.S.C. 552(b)(1), Ex. 1, D.

In the fall of 1959, LASL, in a further attempt to learn about methods of radiochemical sampling for underground testing, core-drilled one of its one-point shots fired in 1957. The results were the refinement of the yield of that shot and the conclusion that such sampling, even long after a test, had great diagnostic value.

By the end of the year, LASL's deep holes had been designed, but ALOO had decided not to go ahead with the construction unless the moratorium situation changed to indicate that the holes would be used. At this point, Bradbury requested that Starbird direct that the 1200-foot holes be drilled, stating that at the moment, LASL probably had the capability of doing a few one-point shots on three months notice, but could not make any statements about larger shots until the holes were drilled or approval was given for their construction.

Livermore was so busy by this time on Plowshare, seismic detection readiness, etc., that they felt forced to back out of the planning of the Jericho shot. They proposed that they be phased out of the management starting Jan.1, so that DASA would have it completely under their control by March 1, 1960.

Radioactive Cloud Sampling, 1959

By mid-1959, although the attention of the testing community had turned largely to the problems associated with underground testing, there were those who felt that atmospheric testing was still a possibility. If the capability to test in the atmosphere was to be maintained, one of the most crucial items was the aircraft sampling capability that had been built up over the long period from 1946 through 1958. The major capability for United States weapons test sampling was in the 4926th Sampling Squadron of the 4950th Test Group in Albuquerque [redacted] This capability was used to sample not only United States nuclear weapons tests at both Nevada and in the Pacific, but also the Rover nuclear propulsion tests. Aircraft sampling required a high level of proficiency on the part of the crews, both in order to prevent radiation overexposures, and in order to assure proper sampling. The sampling tanks used were long lead-time items requiring extensive aircraft modifications which could not be made on short notice. With appropriately equipped aircraft, planning and training for an operation normally began six months to a year before the planned operation.

5USC552 (b)(1) Ex.1, DC

In mid-1958, AFSWC, the owners of the 4926th Test squadron (sampling), had begun the argument that the 4926th strength could not be significantly reduced if a nuclear sampling capability was to be maintained. By December of 1958, Headquarters Air Force had notified the field of its philosophy for continuation of a vigorous program to maintain and improve its atomic capability. Specifically, their guidance was that no actions were to be consummated which could jeopardize or reduce the continued development of their atomic capability, including test resumption immediately following the termination of the test moratorium. This guidance was apparently intended only to instruct all Air Force units to continue pressing for Air Force nuclear weapons. But, it was also used by AFSWC as one of the many crutches to maintain the sampling program. The very fact of the continued existence of JTF-7 during this period of time, and the decision to maintain the Eniwetok Proving Ground at some level, also supplied strong support for the continued existence of the 4926th sampling capability.

Soon after the moratorium began, in spite of the original recommendations, Major General William M. (Monte) Canterbury of the Air Research and Development Command (ARDC), with the agreement of Maj. General C. M. McCorkle, Commander of the AFSWC, somewhat reduced the strength of the sampling group. Both commanders faced the common problem of maintaining an adequate readiness posture while usefully employing the people involved. In this vein they expanded the AFSWC research and development tasks, such as Javelin and Journeyman development and testing, while continuing test support to the AEC on nonweapons tests, such as the Rover program. As another move to strengthen the 4950th the Joint Chiefs of Staff gave approval in December for the formation of a permanent Air Force Task Group, 7.4. That function would be carried out by the 4950th, and would result in increased responsibility for them and for the Commander of Joint Task Force 7. However, this move was never consummated.

In April 1959, the 4950th had eight B57-Bs and two B57-Cs, out of a readiness requirement for twelve B57-Bs and four B57-Ds. The intent now was to convert the Cs to sampler configuration (a useable substitute for the Bs) and to obtain the Ds from Strategic Air Command if they were ever needed. While no particular efforts were made during 1959 to reduce this sampling capability below that established immediately after the moratorium began, it was clear that the organization itself was worried about further reductions if additional jobs were not found. In June of 1959, Colonel Byrne of the 4950th queried both George Cowan of LASL and Ed Fleming of Livermore on the plans and requirements for samplers which could be used from Indian Springs, beyond projects already named. Fleming didn't help the situation when he replied that, in his opinion, other than Rover and Plowshare, there would be no nuclear tests requiring sampling for a long time to come. Furthermore, since Sandia would have completed a Drone Aircraft Sampling System by the end of the year, he did not see that Livermore would need the 4926th sampling capability after that time.

Perhaps 4950th spirits rose a bit when they were told by Merrill Smith of ALOO in September that the AEC was surveying possible canal and harbor sites in Alaska, with a view toward using atomic weapons for excavation purposes in 1961. Participation with AFTAC in exercises in Australia in early October 1959 must also have helped a bit. In October 1959, the ARDC reaffirmed their statement that it was important to retain the 4950th as it presently existed. The possibility of the 4950th continuing to assist AFTAC was strengthened by a rumor in early October that U-2 aircraft might be assigned to the 4950th to support the AFTAC requirements for extremely high-altitude sampling. However, in November, the new commander of the 4950th, Colonel Wignall, was faced with the suggested reorganization of AFSWC, which would do away with the 4950th by absorbing its function into AFSWC Headquarters. Wignall clearly felt that such a move would impair the proper support to nuclear test planning and jeopardize the sampling capability. In December 1959, Headquarters Air Force requested details of aircraft, manpower and schedule requirements to build up the capabilities needed if testing were resumed, the information to be supplied by Feb. 1960.

Thus, at the end of 1959, the capability for sampling atmospheric detonations in Nevada had not been seriously compromised, but the pressure was fairly high to reduce it.

Pacific Test Capability/Willow Planning, 1959

During the Hardtack series in the Pacific in 1958, the Marshallese natives asked the United Nations to take those moves necessary to prevent further testing of nuclear weapons at the Eniwetok Proving Ground. In November of 1958, both John Foster Dulles and Phil Farley* expressed their feelings to the Commission that

*Philip J. Farley was Recording Secretary of the AEC, 1947-1954; member of the State Department Office of the Special Asst. for Atomic Energy, 1954-1957; and then Special Asst. to State Department Secretaries Dulles and Herter.

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further testing in the Marshalls would be most impolitic. However, the testing organization was reluctant to give up the large and familiar plant that had been built up over the years. [redacted] As backup, he also requested that the testing organization look at other possible test sites such as Palmyra, Midway, Canton, or Howland/Baker, and consider open sea operations, using either airdrops and/or Liberty ships as platforms for the devices. The testing organization had to add to Starbird's suggestions the Department of Defense planning for the 1960 Operation Willow.

5 U.S.C. 552 (b)(3) Ex. 3 D.O.E.

[redacted]

5 U.S.C. 552 (b)(1) Ex. 1

The Laboratories took Starbird's directive seriously and sent to ALOO the plant construction and maintenance requirements necessary to continue the EPG test capability. The most important item of maintenance was the signal cable plant, but other repairs, including strengthening a number of the buildings and towers on Parry and Eniwetok, were needed. Money promptly became a problem. In mid-November 1958, Hertford asked for an extra two million dollars to do the necessary construction. Starbird approved some of the work in mid-December 1958, but delayed the rest for further consideration.

In early 1959, discussions between the Laboratory people, Joint Task Force 7, ALOO, and others began to solidify some of these concepts. At a January 28 meeting held at ALOO it was estimated that to achieve an open seas readiness stature would take about five months, but that the Eniwetok Proving Ground could respond in three months. Furthermore an eight-shot operation could be completed at the EPG before any alternate location could be ready. However, later discussions led to the decision to go ahead with the open sea operation concept. The open sea operation would be planned for approximately 600 miles south of Hawaii, using airdrops lifted from Hawaii, or, when the device could not be prepared for an airdrop, Liberty ships as platforms. The operation would be controlled from an AGC command ship. Fireball cameras could be pointed by slaving to the ship's radar tracking system. Sampling would be done by aircraft based at Hickam or Barbers Point. An LSD would be used by the AEC laboratories as an instrumentation ship.

However, in the longer range future, the full Operation Willow, about mid-1960, would use both Johnston and Eniwetok.

In line with these concepts, EG&G began design and construction of the tracking platforms, while the Navy investigated techniques for mooring barges or ships in deep water. ALOO authorized H&N, Sandia, and EG&G to proceed with design and engineering for some of the open sea facilities and timing and firing systems. They began exploration of the use of Pearl Harbor and Hilo as a Hawaiian Test Center. Since the LSD was critical to the open sea concept, ALOO requested that JTF-7 forward the designation and "as built" drawings of an LSD approved for use in the operation in order that the Laboratories could make specific ship modification plans. For their part, AFSWP continued design and construction of the equipment needed for Project Willow, using part of the 17 million dollars in their 1959 budget for that project. The estimate of the total Willow cost was 60 million dollars. No additional funds were yet approved for 1960.

Starbird emphasized that all of these investigations and activities should be conducted in a low-publicity manner to avoid the misinterpretation that we were proceeding toward nuclear testing (in retrospect, it's hard to see how that could have been a misinterpretation). LASL transmitted a number of unclassified messages on this subject in mid-February, and were promptly told by Washington that it was an

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extremely sensitive subject and that it should all be classified.

At the end of January 1959, Parker (AFSWP), noting that magnetic conjugate, communications, and radar effects from high-altitude shots warranted further investigation, [redacted]

5 U.S.C. (b)(1) Ex. 1 D.O.D. and 5 U.S.C. (b)(3) Ex. 3 D.O.E.

In March, the Eniwetok Proving Ground population was 1,800, of which 564 were Army and 407 Air Force. Design and construction work was still continuing, but on a rather leisurely schedule.

In mid-March, representatives of all of the pertinent agencies again met in Washington at Arlington Hall and agreed on the specific planning for conducting an overseas test operation some 300 nautical miles southeast of the island of Hawaii. [redacted]

5 U.S.C. (b)(1) Ex. 1 D.O.D. and 5 U.S.C. (b)(3) Ex. 3 D.O.E.

Scripps Institute was brought in to help calculate the tsunami hazard of open sea detonation, and, by July, had come up with guidelines; one of them was, "For example, if a wave height of six feet onshore at Hawaii is considered the safe limit, a ten-megaton open sea barge shot should be located at least 800 miles away, while a one-megaton shot need be removed only 250 miles."

Thus, during the early part of 1959, the Eniwetok Proving Ground itself was not only maintained, but some repairs were made under the restricted funding. But in early May 1959, at a meeting in Washington attended by high level AEC and DOD personnel, including AEC Laboratory representatives, the conclusion was reached that early resumption of atmospheric tests at the EPG had an extremely small probability, and that, as a consequence, the EPG should be placed on a maintenance standby status and expenditures should be limited to those necessary to prevent deterioration of essential facilities to the point where replacement or repair could cause a long-term delay in test resumption. It was assumed that no tests would commence at the Eniwetok Proving Ground sooner than nine to twelve months after receiving authority to resume testing. At the same meeting, it was concluded that there was a slightly higher probability that open sea testing would be allowed and, therefore, investigation of techniques and planning for such tests were warranted, but that no substantial funds or talent should be expended on the problem.

Just slightly later, during the previously mentioned discussions of the test planning ad hoc group of Starbird, Parker, and Anderson and their subcommittees, several actions were recommended to phase down the resources at the Eniwetok Proving Ground to a maintenance standby status. Some of them were: consolidation of the base camp facilities on Eniwetok Island with concomitant mothballing of the facilities and equipment on Parry Island, which had been the AEC and 7.1 Headquarters; reduction of the boat pool; removal of all locally based aircraft; reassignment of all communications to the AEC contractor (H&N); cessation of any further construction, except for minor modifications needed to consolidate the base camp facilities; and reduction of the total strength to 495 people, of which 69 would be Department of Defense. However, it took a little time to carry out these actions in the field, so in June of 1959 H&N still had under design for the Eniwetok Proving Ground the following interesting items: a new barge slip for Parry Island, permanent reinforcement of the base island buildings, soundproofing and air conditioning of the Livermore and LASL offices on Parry Island, redesign of the IBM computer building, replacement of the triangular photo towers on Parry and Enyu by stronger rectangular towers, and design of a new photo tower to replace the Mack tower. They were also working hard, however, on the open sea concept and had under design an LCU shot vehicle and a more appropriate LSD mast for timing and firing communication.

The Department of Defense continued vigorous planning for Willow through May, June, July, and August of 1959, including support to its contractors for preparation of the mid-1960 operation. However, in early August, the Secretary of Defense, McElroy, changed his guidance, stating, amongst other things, that expenditures for construction equipment and instrumentation for specific tests would be limited to the funds available to the military departments and to DASA for weapons testing purposes. The new guidance stated that no weapons effects series involving overseas operations and environments other than underground would be conducted prior to the spring of 1961. This action resulted almost immediately in a reduction of the funding for DASA projects by approximately a factor of three.

By the 28th of July 1959, the Department of Defense and the AEC had agreed, in principal, that the EPG should be reduced to the minimum required for a 12-month response capability, and the associated DOD organizations were to proceed with corresponding functional and manning changes. (But on the 3rd of August, the Chief of DASA (Parker) added an EPG land surface weapons effects test to the Willow Planning.)

On August 7, 1959, Eniwetok was officially notified by JTF-7 (less than one year after the beginning of the moratorium) that the EPG would be phased down immediately to maintenance standby, with a capability to resume testing within 12 months. Only facilities and equipment which could not be replaced within 12 months would be kept at the EPG and there would be a maximum consolidation of the AEC/DOD functions. Task Group 7.2 was to start immediately to transfer its functions to the AEC contractor (H&N), and was to complete the transfer by January 1960. At the same time, Jim Reeves, wearing the hat of the Commander of Task Group 7.5, ordered the AEC contractor side of the house to proceed in the same fashion, [redacted] Instructions to the Navy Task Group were to return all ships to the Navy with the exception of 16 LCMs and four LCUs which were to be retained at EPG for use by the AEC maintenance and security force.

5USC55 (b)(1) Ex. 1 D.O.D.

So began Operation Switch at Eniwetok. The Laboratories removed all of their equipment from the upper islands and either stored it on Parry or Eniwetok or brought it home. Military-owned equipment, except for that needed to keep the airfield open, was either transferred to the AEC or returned to CONUS. Coaxial cable was returned to the Nevada Test Site. The Air Force redeployed all helicopter personnel to home station. By mid-December 1959, the Task Force strength on Eniwetok was down to about 20 from a level of 1,000 at the beginning of the year, and Holmes & Narver had taken over virtually all functions. By late January of 1960, Operation Switch had been completed.

In parallel with these actions at Eniwetok, the Task Force support organization, consistent with the recommendations made through June and July, was also phased down. As mentioned elsewhere, the Technical Task Group, 7.1, was deactivated as of August 31, 1959. JTG 7.2, the Army housekeeping organization at Eniwetok, as previously mentioned, was continually reduced in size and finally moved from Eniwetok to Arlington Hall Station on January 17, 1960. Task Group 7.5, the AEC Task Group was deactivated, but retained a fair fraction of their personnel within AEC Field Offices, at ALOO, or in Nevada.

The previously mentioned study by Chief AFSWP, JTF-7 Commander, and Starbird recommended that JTF-7 become a subordinate command of DASA. That action became effective November 27, 1959, with the previous JTF-7 Chief of Staff, Brigadier General George T. Duncan, taking over command from General Anderson. On August 20, 1959, Duncan had indicated to the Department of the Army that there would be no requirement for a general officer in the Task Force after FY 1960. Rear Admiral Parker, Chief DASA, on November 9, directed that a plan be developed to transfer the Nevada Test Site support functions of DASA to JTF-7, and that JTF-7 establish an Albuquerque office to carry out that work.

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By the end of 1959 a major portion of the organizations for conducting overseas tests had disappeared. Eniwetok Proving Ground had been put in a caretaker status, the plans for an open sea operation had come and gone, and virtually the only active planning for a real operation was the DASA planning for Operation Willow, to be conducted in 1961.

High-Altitude Test Detection, Mid-1959 Through Early 1960

As previously related,* the Conference of Experts had recognized their lack of knowledge concerning the detection of high-altitude detonations and the need for a later conference on the subject. Efforts ** were made in early 1959 to prepare for such a meeting, including the appointment by PSAC of an Ad Hoc group on High Altitude Detection, chaired by Dr. Wolfgang Panofsky.

The Commission began considering the high-altitude detection question in earnest at a May 29, 1959, meeting, at which they were briefed by Dr. Richard Latter of Rand. The reasons for conducting high altitude nuclear tests, Latter said, are as follows: (1) to obtain further information on the scientific and military applications of nuclear weapons; (2) to carry out tests, other than underground, which would not result in radioactive fallout; (3) to continue testing in the event there is agreement to limit testing at altitudes below 100,000 km, thereby avoiding radioactive fallout; and (4) to determine the capability to carry out and detect clandestine nuclear tests above 100,000 km altitude in the event of agreement to cease all nuclear weapons tests. He summarized the Panofsky Panel's conclusions as being that nuclear testing is feasible at altitudes up to 300 million kilometers, that it is feasible to establish a system of satellites for detection [redacted] In response to expression of doubt about a fully guaranteed high-altitude detection capability, Latter said that he could not detail such a system at the time, but felt that an adequate system could be established by 1963. Through this briefing, and one a few days later which included some new information from the Panofsky Panel, the commissioners concluded that high-altitude test detection was more feasible than underground test detection and also reached a consensus that the results of the panel's studies and reports should be made public.

5USC.S: (b)(1, Ex. 1 D.O.E.

Khrushchev expressed a willingness to join in technical discussions on the high-altitude detection problems as proposed by the U.S. and U.K. Thus, a meeting of experts from the three countries convened on June 22 in Geneva, concluding their talks with a final report on July 10, 1959. This group, known as Technical Working Group I, made the following general recommendations: that five to six earth satellites be emplaced at altitudes greater than 30,000 kilometers for detection of neutrons, prompt gamma rays, delayed gamma rays, and soft x-rays, or, if technical or economic reasons required, this system be deployed at low altitudes; that a satellite be placed in the appropriate elliptical earth orbit to cover the magnetic field regions of electron trapping; that, if thought necessary, a system of four solar satellites be emplaced to increase coverage of the regions behind the moon and the

*In a May 14, 1959, letter (*Department of State Bulletin*).

**An early example was an April 19, 1959, report written at LASL by Don Westervelt, in draft form, on an atmospheric fluorescence system. Major Robert Fisher of AFTAC had requested that this report be submitted immediately, even though it was only partially complete, based on an urgent request from Hans Bethe that it is needed in Geneva. Thus, three copies of the hurriedly finalized draft were sent to Geneva to Spurgeon Keeny, Bethe, and Panofsky.

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sun; and last, "that ground control posts be equipped with instruments for observing direct visible light, for observing fluorescence in the upper atmosphere, for measuring the absorption of cosmic radio noise in the ionosphere, and for measuring radio signals."

During the summer of 1959, the DOD had agreed to accept overall responsibility for high-altitude detection, but there was uncertainty as to who would oversee the work until the primary role was given to ARPA on September 2. Until this decision was final, between about April and September, the lack of definite responsiblity caused some problem, although it is clear that AFTAC was playing the major DOD technical role at this time.

On July 22 Starbird requested that the LASL and Sandia directors produce a concrete statement of the work that had to begin without delay, for forwarding to AFTAC. AFTAC needed the details to formulate the final program in order to ask for approval and funding from the DOD. Since AFTAC felt that this funding might be a long time in coming, DMA asked for emergency funding for its portion.

The U.S. satellite borne detection efforts, which were based on the work already being overseen by the Buzzer Committee, were to become known as Vela Hotel. The techniques of surface-based detection of high-altitude explosions, such as the observation of nuclear explosion-induced atmospheric fluorescence being worked on at LASL, became part of Vela Sierra.

During the summer, some of the detectors and logic systems being developed at LASL and Sandia for satellite packages were tested. A neutron detector, an electron magnetic spectrometer, and a proton counter telescope were flown on small rockets as the beginning of experimental work on such systems. Doyle Northrup (AFTAC), on September 3, 1959, forwarded to Bradbury a background program document entitled "Proposed Program of Research on Detection of Nuclear Explosions at High Altitude in the Atmosphere and in Space." The document discussed Technical Working Group 1 recommendations of July 10, 1959, to the Geneva Conference, and addressed the feasibility of various methods of detecting high-altitude explosions using ground stations and satellites. Northrup stated that AFTAC had the overall technical project management whereas ARPA would be responsible for overall supervision and funding. AFTAC intended to request the assistance of consultants in various disciplinary fields, and proposed the establishment of an advisory panel to DDR&E. The proposed panel would have Panofsky as the chairman and Dick Latter as the acting chairman, with Bethe, Bing, Donovan, Goldberger, Longmire, Molnar, Peterson (SRI), Pickering (JPL), Taschek, and Watson (LRL) as members. Part of the program was to be a set of nuclear tests to check the detection system's capability. The tests would include a repeat of the last Argus test and a number of other tests, with and without x-ray shields, between 100 thousand and 300 million kilometers altitude.

The AEC laboratories were asked late in the summer to provide information on their theoretical work to the DOD, who also had the Army Ballistic Missile Agency (ABMA) and the Space Technology Laboratory (STL) on contract to ARPA to provide information to the Department of State for the Geneva negotiators. The Geneva negotiators requested that ARPA study and evaluate detection systems for explosions of one kiloton or larger yield, at altitudes above 30-50 kilometers, using either surface or satellite means. ARPA was to provide a report to Geneva by October 7, 1959, and to update the report within 6 months.

In early fall 1959, after being assigned overall responsibility, ARPA issued Order No. 102-60 directing Air Force Ballistic Missile Division (AFBMD) to investigate a system of ground stations and satellites for detecting nuclear detonations at altitudes above 50 kilometers. By Amendment 1 to the same order ARPA directed BMD to produce a development plan for the R&D program leading to a detailed definition of such a satellite system. This plan was to be developed by a joint working group including AEC, NASA, and ARDC. The group's report was published in rough draft in March of 1960 by the Vela Joint Working Group, chaired by Colonel Harry Evans of AFBMD. The report, entitled "Project Vela Hotel, ARPA Order No. 102-60" discussed all aspects of the proposed satellite systems (using a large part of the Buzzer Committee's October 1959 report, entitled "Capability Report for a Satellite System for Nuclear Burst Surveillance"), as well as associated communications, launch vehicle development, possible use of various rocket probes for instrumentation development, and extensive estimates of cost. Sixty-three million dollars would be required between FY 1961 and 1964, 36 million for launch vehicles and 20 million for satellite payloads.

In October, at Starbird's request, both LASL and Sandia estimated the FY 1960 funding and manpower required to support Vela Hotel. LASL required additional funding of something less than $1 million, and Sandia estimated $2.7 million, more than half of which was for hardware procurement. On October 29, Glen Fowler of Sandia updated the Sandia cost estimate to a significantly lower number for FY 1960. Livermore, seldom mentioned in high-altitude detection, responded with an estimate for a small effort of $200,000.

On October 29 Taschek gave Don Shuster LASL's rocket support requirements. LASL required, between January and July of 1960, about 10 small rockets having a 100-mile altitude capability, and about 10 carriers with a 300 mile altitude capability as soon as Sandia could make them available. For higher-altitude applications, LASL and Sandia would jointly request carriers from AFTAC.

On November 23 Starbird gave LASL a go-ahead for their program; but authorized Sandia to proceed only with general research, not to include any hardware purchases.

On February 19, 1960, Hertford summarized the LASL and Sandia requirements for Vela Hotel for the next several fiscal years. Sandia intended to utilize 10 Nike-Cajun rockets for instrumentation flights thnrough the rest of FY 1960. Journeyman B rockets for instrumentation flights needed by LASL and Sandia were to be provided by AFSWC. Other instrumentation packages were planned to be carried piggyback on NASA satellite flights. Finally, emplacement of four Vela Hotel satellites was planned, with the first pre-prototype package of a nine-satellite build to be launched in 18 to 24 months.

Deep Space and High-Altitude Nuclear Testing, Spring 1959 Through Early 1960

April 1959 hearings on the Argus explosions by the House Committee on Science and Astronautics and the subsequent publication of the unclassified part of these discussions in June served to increase awareness of the possible future use of space for nuclear test detonations. Public reaction to the problem of atmospheric fallout from low altitude testing was one of the factors that had led to the test moratorium, so there was need to consider the possibility of deep space nuclear weapon testing as an alternative or supplement to underground testing. There was need to consider the methods by which the enemy might cheat on a CTB by testing in outer space. There was need for the DOD to understand the effects on communications, radar operation, etc. of nuclear detonations at high altitudes. These needs led to growing attention to this testing environment in the spring of 1959. As mentioned elsewhere, planning for the DOD high-altitude effects series Willow had been going on for many months, but only in early May did the Commission (AEC) begin addressing the possibility of satisfying some of their own requirements by testing in the upper atmosphere or in deep space.

In May, Starbird requested that the Laboratories send him information on possible high-altitude testing, detection, and evasion methods, in preparation for the

[redacted]

upcoming Technical Working Group I meeting. He especially desired a Livermore report on possible shielding of high-altitude nuclear detonations. Molnar (Sandia) responded in late May with details of how nuclear devices and rocketborne experimentation could be utilized to test in outer space, with particular attention being given to those details that Molnar felt should not be discussed with the Russians at the upcoming High-Altitude Detection Technical Working Group. He forwarded two studies on high-altitude testing possibilities, addressing methods of positioning and instrumenting such tests, the basic measurements that would be sought, and how they would be obtained, and discussing safety problems, both with the missile and the warhead, and how these problems might be solved.

The Commission was briefed July 17 by General Starbird on the various preliminary high-altitude test summaries and proposals. Their reaction was to regard the information as extremely sensitive, and to suggest that the reports ought to be closely held.

The DOD, through Loper, suggested to the AEC that Willow be made a joint AEC-DOD program, but in the summer of 1959 there was neither a strong desire by DASA to have the Laboratories' programs fully included in Willow, nor an eagerness by the AEC technical people to include their requirements in this DOD series of tests.

DASA planning for the high-altitude portion of Willow, as updated to the DDR&E on August 3, was to carry the appropriate warhead aloft on a Jupiter missile launched from Johnston Island. The missile would also carry two to four "pods" to be deployed for close-in measurements. Companion rockets would be utilized for additional measurements. A review of the Willow plan by DDR&E resulted in a decision in August to conduct Willow no earlier than March 1961, and the JCS was given the figure of two million dollars to fund the FY 1960 effort rather than six million as originally recommended by AFSWC. AFSWC was to be in charge of the rocket launch and pod programs, as well as small rocket programs for other DOD Laboratories.

The Airforce Ballistic Missile Division, on their own initiative, published, on September 1, a proposed program for Outer Space Weapons Testing. Their report began by noting the Air Force "realization that the U.S.S.R. has such a capability and may well be in a position to exploit it." A carrier system was proposed to lift a 1,000-pound payload containing the device, measurement sensors, and equipment to transmit the data back to earth. The time to have a new test capability was estimated to be greater than 24 months. The Eastern Test Range and Johnston Island were considered possible launch sites for the program, estimated to cost around $30 million.

The first detailed proposal from the AEC Laboratories on a deep space test capability came from Edward Teller to Starbird on September 30, 1959. Teller encouraged development of that capability and proposed use of a three-stage Atlas booster launched from either Eniwetok-Bikini or Christmas Island. Johnston Island, he felt, should not be considered because it was already overcrowded. Livermore estimated that this development would take about 18 months (through the first calibration shot) and cost about $50 million. Later shots would cost $10-15 million each. Teller stated that either LASL or Livermore should be assigned responsibility for the experiment, but that Livermore could not undertake the job without an increase in staff and, thus, he preferred that LASL undertake the job. McCone notified Starbird on October 18 that he was impressed with Teller's proposal.

The earliest mention of the Thor as a device carrier is in an October 27 AFSWC proposal for inclusion of Argus type experiments in Operation Willow. AFSWC recommended that three tests be carried to altitudes of 100 to 400 kilometers (from Johnston Island) by the Thor (which would carry instrumentation pods), with additional diagnostic equipment to be carried by Javelin rockets.

Discussion of these various proposals had progressed sufficiently by late 1959 that the Chief of DASA, Admiral Parker, sought General Starbird's concurrence that

[redacted]

[redacted]

DASA fund preliminary planning for development of an outer space testing capability. Starbird forwarded the DASA proposal for joint consideration of outer space testing to ALOO and the weapons laboratories on January 19, 1960. Noting the similarity between the AFBMD and the Teller proposals, Starbird asked for opinions about a reciprocal program with the DOD whereby the AEC would provide the warhead and some diagnostic packages for the two highest-altitude Willow tests, while at the same time encouraging the DOD to proceed with planning and engineering studies outlined in the AFBMD proposal. Starbird noted that "except for organizational concept and assignment of responsibilities," the DOD outer space program would accomplish the purpose of the Livermore proposal. That particular exception was one that was extremely important to the addressees. Gerry Johnson of Livermore replied, on January 26, that there was a clear difference between the objectives of Willow (high-altitude effects) and outer space testing (to develop an AEC lab capability in that regime). Furthermore, Livermore specifically disagreed with Starbird's proposed division of responsibility, feeling that the development of any testing capability was the responsibility of the AEC, and military participation would appropriately be only supportive. Bradbury, although somewhat milder, was also pessimistic about the organizational format and responsibilities. He also felt that the AEC must retain control. The Sandia reply, on January 27 was even stronger in guarding the AEC's rightful responsibilities on Willow and outer space testing. Sandia felt that DASA should be notified that the AEC would provide a package containing warheads and associated arming, firing, and diagnostics systems for joint AEC-DOD Willow tests for both weapons effects and diagnostic purposes. Furthermore, for the outer space test capability, DOD might provide and control the launch vehicle and perform certain other functions such as site selection and preparation, but the AEC should have overall test direction to ensure that the scientific objectives of these tests were met.

After receiving these replies, Starbird informed Hertford, with information copies to the Laboratories, on February 3, 1960, of his response to DASA. He indicated that the AEC agreed to provide the warheads and diagnostic packages for the Willow shots. Starbird felt the organizational responsibilities could be worked out later, following approval of the tests. As for the outer space testing proposal, Starbird recommended that DOD proceed along the line of the AFBMD study. The Laboratories were to proceed with engineering and developmental planning for an AEC package which would contain the warhead, arming, fusing, firing, safing, diagnostic equipment, and related telemetry for a full-scale nuclear test.

Based on Starbird's agreement DASA released funds to ARDC for further study of their outer space testing proposal, in coordination with the AEC. The technical working group that was formed to study this was chaired by Air Force Ballistic Missile Division (AFBMD, under ARDC) and had representatives from AFSWC, Livermore, LASL, and Sandia. At their first meeting on February 19, 1960, at AFBMD the group agreed to develop a detailed plan on a testing capability for some appropriate distance outside the earth's influence. Following approval by both AEC and the Air Force, the plan would be delivered to DASA by July 28, 1960. This system was given the acronym ASWT, for Advanced System for Weapons Test. Early discussions indicated that while it would cost an additional $30,000,000 to launch from Johnston Island instead of the present facilities at Cape Canaveral, it would also solve a number of operational and safety problems.

In January 1960, DASA canceled Redstone flights planned for later that year to test the missile with the instrumentation pods, indicating that such tests were an unwise investment in the Willow low-altitude program. Virtually all of the Willow activity and active planning and preparation were discontinued by DASA February 26, although Task Group 7.3 (Navy Task Group) supported a series of tests off the Florida coast in February and March of 1960 to evaluate various methods of locating and recovering pods ejected from missiles on high-altitude tests.

Thus, after all the studying and coordinating of proposals, a couple of months into 1960 the only firm high-altitude program (Willow) was completely inactive, but the AEC and DOD had started to coordinate planning and development of an outer space test capability.

Underground Detection, 1959

On April 23, 1959, Chairman McCone met with Killian, Quarles, and Starbird and agreed that the AEC would be primarily responsible for following the Berkner Panel recommendation to undertake an experimental test program to determine the parameters of detection and concealment of underground nuclear detonations, and to determine the feasibility, practicability, costs, and timing of underground tests necessary to investigate these parameters. The AEC and the DOD would try to establish the test requirements jointly.

Livermore and Rand, working with AFTAC and Sandia, promptly set about to carry out these aims under the guidance of such people as Carl Romney of AFTAC, Glen Werth and Harold Brown of Livermore, and Al Latter of Rand. In May of 1959, there was pressure on Livermore to produce results quickly, especially on the Latter-hole concept. But Harold Brown, in an exchange with Starbird, made the strong point that it would be better to take a year and get the correct results than to produce erroneous results in a short time, even though results were needed for the upcoming meeting of Technical Working Group II at Geneva.

In late June, an interagency "Scientific Panel to Evaluate the Overall Adequacy of Test Detection Systems" was set up, and as part of that, the Ad Hoc Group on Seismology, with Dr. Frank Press as Chairman, was formed by the Director of Defense Research and Engineering. The Commission, at Starbird's suggestion, made Harold Brown the AEC representative, with Spofford English and Starbird as observers.

The feeling that data had to be produced to assist in the test ban negotiations grew stronger and stronger in mid-1959. This desire for data grew out of two opposing viewpoints. Those who felt very strongly that continued testing was to the benefit of the United States wanted such measurements and calculations in order to show that detection and identification systems would really not be effective against a determined cheater. Those who felt a treaty, a cessation of all testing, would be to the benefit of the United States also felt that such measurements and calculations helped develop confidence that the U.S. would not be trapped by allowing the Soviets to gain information from clandestine testing, which they could do, whereas we, on the other hand, would not advance any further because, in our open society, it was clear that we could not, and would not, conduct tests clandestinely.

There was, of course, a third set of people who wanted the cessation of testing, believed the Russians would not cheat, and thought that the whole business of requiring on-site inspections and detection stations within the Soviet Union was a lot of folderol. However, their voice was not loud in Washington. Obviously, persons of this third category did not believe that either further experimental or theoretical investigations were necessary. However, since that happened to be the Russian line at the moment, it was not popular in American circles.

The general line of attack seemed to be to produce data and theory to convince the Soviets that the problem of detection was difficult and that cheating was possible. This line was apparently supposed to convince them that they should accept our proposals for on-site inspection and international detection systems, or at least that they should join us in designing satisfactory systems of that type. In essence, we seemed to be saying that "We know we won't cheat, but we know you will if given the chance, so why don't you woo us into a complete test ban treaty by allowing us to design and install methods to prevent any cheating that we can imagine. And furthermore, we would like you to help us pay for this."

As had been remarked by the Berkner Panel, the overall problem of underground detection involved an understanding of the response of the earth to nuclear detonations and earthquakes; improved seismological techniques for investigating these phenomena; and an understanding of the effects of geology, depth of burial, etc., on coupling of nuclear detonation energy to the earth, as manifested by the appearance of that energy at teleseismic distances. It was of great importance to distinguish somehow the signal of a nuclear explosion from that of an earthquake. To offer significant improvements in the detection and identification system, it was necessary to learn, by theory and experience, a great deal more about the signals from nuclear detonations.

The Latter "big hole" theory prediction of decoupling factors as high as 300 was addressed by means of two programs, one high-explosive and one nuclear. The high-explosive program, Project Cowboy, consisted of several shots fired in hollow cavities in salt. In particular, 1,000 pounds of high explosive was fired in the center of a 30-foot diameter spherical cavity and the seismic signal from that compared with the signal from the detonation of a 1,000-pound high explosive closely tamped in salt. The seismic signal (at several frequencies) was observed at distances as great as 44,000 feet. Between December 1959 and mid-March 1960 these experiments showed that the decoupling theory was approximately correct for high explosive. Amongst our own experts there was not agreement that high-explosive experiments could prove that the Latter decoupling theory applied to nuclear detonations.

The second part of the cavity decoupling program, nuclear decoupling, was the subject of Commission discussion in late July. As a result Starbird notified the Laboratories and AFTAC on July 24, 1959,* that Chairman McCone wanted to accelerate studies and actions to stage those underground nuclear shots necessary to confirm or refute the Latter decoupling theory. On August 3, 1959, at a Washington meeting called by Starbird, attended by Northrup and Romney of AFTAC, Al Latter of Rand, Carson Mark of LASL, and others, an attempt was made to further planning for the decoupled and associated closely tamped nuclear events. After arguments in which Livermore suggested 200 tons yield, and AFTAC 5 kilotons, agreement was reached to attempt a 1.7 kiloton detonation in salt. If it turned out to be impractical to build a cavity for that yield the 200 ton yield would be the fallback position. The decoupled shot could presumably be conducted by March of 1960.

Early on, it was recognized that the coupling of the energy of an underground nuclear detonation to the earth would be dependent upon the medium in which the shot was fired (alluvium, tuff, granite, salt, etc.).** In order to investigate this phenomenon and the question of nuclear vs. high explosive coupling, Project Concerto was initiated. By early August 1959, the engineering was well along for that project, to be conducted at the Nevada Test Site. Project Concerto involved some seven closely tamped shots, of which six were to be nuclear. Specifically there were to be three 5-kt nuclear shots (Orchid at 2- to 3,000-foot depth in tuff, Porpoise at 10,000-foot depth in tuff, and Dinosaur at an unspecified depth, but off site); two shots to compare nuclear vs. high explosive at 1 kt (Cottontail, high explosive in

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*A handwritten comment on the LASL copy of this message is "How come the Chairman will let this test be planned, but Starbird says 'nothing doing on one-points'?" Surely getting ready for a decoupling shot will be "obvious" and arouse comment.

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tuff, and Crystal, nuclear in tuff); and two other shots to look at the effects of yield (Coffee Pot, 1/4 kt nuclear in tuff, and Stingray, 50 kt nuclear in tuff). In mid-September, Harold Brown (Livermore) requested authority to proceed with construction for the high-explosive Cottontail event to achieve a February 15, 1960, ready date for the shot. Of the whole Concerto program, this was the only shot he felt could be conducted without serious political restrictions. Starbird again stressed the need to avoid any leak or speculation that the large hole experimentation was indicative of AEC planning for any nuclear tests, even though discussions had been going on with the Russians for some time aimed at getting them to join these experimental efforts to improve the definition of the required worldwide seismic net. In early October, Gerry Johnson of Livermore again requested from Starbird authorization to proceed with the engineering and construction for Cottontail. However, Livermore had chosen a new site in a separate tunnel and now proposed a new readiness date of May 1, 1960, which would be consistent with the AFTAC readiness to observe teleseismic signals from Lollipop,* intended for April 1, 1960. On October 22 Starbird stated that, following ALOO review, the new Cottontail emplacement plan would be discussed with AFTAC. In November 1959, the Commission reviewed the Lollipop plans, wishing to be sure that any instrumentation proposed could be revealed to Soviet representatives if the test should be conducted under international sponsorship. There was worry that some of the proposed projects associated with this shot could not be defended solely on the basis of seismic detection studies of underground nuclear shots, but appeared to aim more toward the development of weapons testing techniques and nonmilitary uses of nuclear explosions, [redacted]

On September 2, 1959, Project Vela came into existence officially with the assignment of the seismic, high-altitude, and surface detection programs to ARPA. The underground detection study program became known as Vela Uniform (Uniform for underground). However, the AEC continued to carry the responsibility for the nuclear underground detonations and for Cottontail.

Withheld Under 5 U.S.C. 552 (b)(1)

By the end of 1959, a great deal of the effort at Livermore, Sandia, and the test section of ALOO had been transferred from weapons testing to the design and preparation of underground shots for seismic detection purposes. An appreciable part of the FY 1960 weapons funding had also been transferred to that purpose.

Plowshare, Late 1959

Livermore and the Commission, in particular Libby, continued to press hard for some arrangement that would make the Plowshare program feasible, either during the moratorium or under the framework of any treaty to be negotiated. There was a bit of schizophrenia at Livermore. On the one hand they were pressing hard for a provision that would allow underground testing, even though atmospheric testing was banned (a variation of this had been proposed at Geneva in April), and on the other hand they were also pointing out (Brown to Starbird, April 22) that being allowed to do the most obvious cratering shots was in conflict with the proposed requirement to test only underground - it's hard to make a crater and yet completely contain. Another problem that received serious attention was that of making an unclassified Plowshare test area, that is, an area available for inspection, so as to prove that it had nothing to do with weapons development.

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*Separately planned by Livermore and AFTAC (in response to the Berkner Panel recommendations) as a 5-kt tamped nuclear shot in granite at the NTS.

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During the summer of 1959, Livermore settled on a number of Plowshare proposals, of which the most significant were:

a. Gnome - planned as a 10-kt detonation in the Salado Salt Basin about 25 miles southeast of Carlsbad, New Mexico. The detonation would be some 1,200 feet below the surface. The primary purpose of the experiment was to investigate the use of such an explosion as a source of heat or energy to run an electric power plant (although no plant was to be associated with the detonation). Side issues were the investigation of the production of transplutonic radioisotopes and additional data on seismic coupling in salt.

b. Chariot - an excavation project to produce a harbor close to Cape Thompson, Alaska.

c. Oxcart - which was to be a couple of detonations at NTS to investigate excavation efficiency as a function of yield and depth to assist in the planning for Chariot.

d. Ditchdigger - was to be the test of a [redacted] clean explosive device which would enhance the feasibility of such projects as the proposed sea level Panama Canal.

e. Oilsands - an experiment to study the feasibility of oil recovery by means of a nuclear explosion in the Athabascan tar sands of Canada.

f. Oil Shale - which would use a nuclear explosion to shatter an oil shale formation, followed by an attempt to retort in place.

Withheld Under 5 U.S.C. 552 (b)(3) DOE, Exemption 3

By September of 1959 Livermore had presented the first four of these to DMA as specific project proposals; however, Plowshare could clearly not proceed without some clarification on the test moratorium question. There was strong fear that the budget would be reduced by Congress unless the promise could be made that some projects could really be carried out. The Commission and General Manager were faced with funding limitations and the requirements of the readiness and underground detection programs, so that they, too, were questioning the proposed Plowshare funding. In early September, Starbird requested that his staff (Kelly and Keto) proceed with those steps necessary to activate "the Plowshare Advisory Committee," with the aim of a first meeting in October. It was his feeling, later concurred in by Teller, that the recommendations of a properly formed high-level committee on the subject would bear more weight with those judging the program than the recommendations of specific concerned individuals. Starbird considered (September 1959) the Gnome shot to be somewhat questionable technically and wrote "Three million dollars may not appear much money in a weapons program, but in a congressional investigation, particularly in an election year, it could loom mighty big as an agency waste." He felt that the joint AEC and Bureau of Mines Oil Shale shot might be the white hope to keep the program going. He was somewhat unhappy about Oxcart because it had no public appeal in demonstrating, visibly, new uses, since it was at NTS and would be regarded with suspicion as a weapons test. He felt that Ditchdigger was important to the long range future of Plowshare, but also felt that some treaty would have to be agreed to before an approved effort could be achieved. He specifically felt that Chariot had the greatest long range potential. All of this he intended laying before the to-be-formed Plowshare Advisory Committee.

In January of 1959, Wadsworth, in Geneva, in an attempt to get a "Plowshare Exception" to the proposed CTB Treaty, had tabled a "Black Box" proposal suggesting that devices to be used for peaceful purposes should be placed in a repository on or before the date the treaty was signed and kept under continual surveillance until used. These devices would not be subject to internal inspection, but if any other devices were to be used for peaceful purposes, they would be subject to internal inspection. The Soviet Union initially rejected the concept of a "Plowshare Exception," but later agreed that some explosions could be conducted if the other side were given the right to inspect the internal structure of the devices used. They also demanded parity between the two sides on the number of nuclear explosions. At home there was a great deal of discussion on what devices might be placed in such a stockpile. By mid-September of 1959, "what if" detailed discussions were going on on the possibility of using versions of the Mark VII for this purpose. LASL expressed confidence that various yield versions would be quite reliable, even in the low-yield range, without further test. However, at the same time, Starbird and Luedecke, recognizing "it was difficult to make a budget recommendation on this matter because of the status of the Geneva Conference and its effect on the expenditure of FY 1960 funds available," nevertheless recommended eight million dollars for the FY 1961 Plowshare budget to permit flexibility in the event the program should go ahead.

Laboratory Weapons Programs, 1959

During this time, the Laboratories were reconsidering in more detail their weapons test requirements and the question of how much device design work could be done under the moratorium. At the previously mentioned meeting on May 7, 1959, between members of the Department of Defense, representatives of the weapons laboratories, and DMA, there was discussion of programs for which testing was desirable. Specific development programs listed were a 600 pound maximum yield warhead for Minuteman and Polaris; a 3,000 pound maximum yield warhead for Atlas and Titan; a 300 pound, [redacted] warhead for Pershing; and a 1,600 pound warhead of maximum yield for possible future ICBM use.

Of perhaps more interest than the specific programs were the concepts presented at that meeting which might be developed or investigated through further testing. [redacted] Further improvements in the safety against nuclear contribution to the yield of accidental detonations seemed both possible and essential. Most controversial, however, was the introduction by Livermore of the concept of enhanced radiation warheads for specific kill effects, of which the most immediate possible development seemed to be enhanced neutron output devices. The conversation here on enhanced neutron output was to lead later to the newspaper concepts of the neutron bomb, which would kill people or other animals by neutron exposure, but would produce no heat or blast and, hence, would destroy no material. [redacted]

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The proposed NTS underground program agreed upon at that same meeting followed some of these concepts. [redacted]

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[redacted] 1959, the realities of the situation began to impress themselves on the system. Washington did not agree to all of the construction requested by the Laboratories, and readiness for any large underground program was beginning to slide off into the more distant future. Furthermore, there had been time to look more carefully at what the problems really were. [redacted]

Withheld Under 5 U.S.C. 552 (b)(3)

Summary of 1959

Thus, by the end of 1959, the testing system had pretty well convinced itself that any future testing would either be underground or in deep space (above 50 kilometers). Both proving grounds had been degraded to some extent; Nevada in the sense that funding for desired readiness construction was not being made available, and Eniwetok as a conscious move to reduce to a 12 months readiness condition. Money for readiness had become very tight because of the possible need to carry out programs that might be allowed during the moratorium or by treaty terms; that is, Plowshare, investigations into the detection and identification of clandestine shots, and methods of evading detection systems. The testing organizations had been degraded, some not seriously but others very seriously. Eisenhower had concluded that it was a good gamble to continue the test ban discussions and the moratorium beyond the one year initially stated.

Geneva and Other International Developments, Early 1960

As discussed above, President Eisenhower had changed the U.S. test moratorium policy at the end of 1959, leaving us free to resume testing if we decided that to be the proper course and obligating us only to make a prior announcement if we resumed testing. Following Eisenhower's December 29 statement, Khrushchev declared, in a December 30 interview, that the Soviets, having already suspended all nuclear weapons tests, would not conduct any more unless the West did so. As a result of the U.S. position on the lack of enforceability of a comprehensive treaty, the U.S. brought to the conference table on February 11 a treaty proposal to ban all controllable and detectable nuclear tests. Specifically, it called for a ban on atmospheric, underwater, and high-altitude tests at altitudes for which effective controls could be agreed to, but limiting the underground test ban to a seismic magnitude of 4.75 or higher. The proposal also called for a joint program of seismic research and experimentation and proposed to systematically extend the underground test ban until adequate control measures could be agreed upon. Having rejected the U.S. treaty proposal in February 1960, the Russians made public a counterproposal on March 19. The essential changes proposed were that all sides agree to a moratorium on underground tests below 4.75 seismic magnitude beginning on the date of treaty signing, and that the joint research program not specifically allow for nuclear tests underground. The

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Western reply to this Soviet proposal resulted from a meeting of British Prime Minister Macmillan and President Eisenhower in late March. Their joint statement of March 29 offered to accept the Soviet proposal if the test moratorium were to have a fixed duration and if the joint research program were agreed to before signing, provided that the remaining treaty issues, including inspection, were resolved. Eisenhower also stated that such a moratorium on low-level tests would be a personal agreement and that he could not obligate his successor. The Geneva negotiators agreed that technical talks on the seismic research program would be held in May in parallel with the political negotiations. The Soviets made a further concession on May 3 that the program could include a "strictly limited number" of nuclear experiments, formerly not allowed by the Soviet proposals. Eisenhower took this opportunity to publicly announce the Vela program.

Thus, by May 1960, from the setback felt by some at the lack of progress made in Technical Working Group II, the events following the U.S. phased treaty proposal had increased the hope of the optimists that a real signed test ban treaty was in the offing. In Eisenhower's own words,*

> From the autumn of 1959 to the spring of 1960 most people of the Western world felt that a slight but discernible thaw was developing in the icy tensions which had become normal between the West and the Soviet Union. This impression resulted partially from Mr. Khrushchev's agreement at Camp David to remove his threat to end the presence of Allied forces in West Berlin. His action made it possible for the Western nations in December to agree to attend a summit meeting without sacrifice of self-respect and under no hint of blackmail. Plans for a spring meeting began with a place and date: Paris, in mid-May of 1960.

Curiously, many of the technical experts were convinced that an underground detection system might never be effective below the thresholds already discussed, even following extensive seismic research testing. Certainly, many felt that the seismic detection system would not be cost-effective based on the estimated enormous cost of installing and operating such a system. In addition, many doubted that an effective agreement on inspections could ever be reached. At any rate, public hopes that a test ban treaty might be signed were somewhat higher in May 1960 than they were at any other point during the moratorium.

Out of the mainstream of these negotiations, but of interest, were the initial discussions within the Commission on a threshold treaty on January 11, 1960. At this meeting, Spofford English, who had attended a meeting of the Principals on December 28, 1959, reported that the threshold treaty was being discussed and that George Kistiakowsky (who had replaced Killian as the President's Science Advisor) and James Fisk were assembling a group of seismologists to study the question of a threshold limitation and to define a workable yield figure based on seismic signal magnitude. The Commission discussed the meaning of seismic magnitude threshold in terms of yield and impact on AEC weapon development progress. DMA informed the Commission that seismic magnitude 4.75 was equivalent to a 19-kiloton coupled detonation and magnitude 5 was equivalent to about 43 kilotons.

Withheld Order
5 U.S.C. 552 (b) (3)
DOE, Exemption 3

*Eisenhower, *Waging Peace*, page 546.

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It is of interest that later in the spring, at the Geneva discussions on the threshold treaty proposals, the Russians were equating a 4.75 seismic magnitude to about 5-kilotons, as stated by the 1958 Conference of Experts, rather than 20 kilotons as the U.S. was now estimating.

Reflecting the Administration's thinking while the threshold treaty was being tabled and discussed, Kistiakowsky wrote to Chairman McCone on February 24 that the President wished McCone and Secretary of Defense Gates to be informed that he considered a "vigorous and continuing research and development program on the detection of underground and high-altitude explosions to be a matter of high priority and that he hopes the DOD and AEC will find it possible to finance this program for FY 1961 within their existing budgets." (Perhaps this has a direct tie to the fact that Starbird was trying to get all of the increased readiness activities paid for out of FY 1960 funds, since he expected a problem with procuring FY 1961 funds because of the high priority to be given to Vela.)

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5 U.S.C. 552(b)(1)
1.3(a)(4), Exemption 1
DOE

NTS Weapons Test Readiness, Early 1960

During early 1960 the discussion between Livermore and Starbird (DMA) on a Livermore readiness program continued, mainly concentrating on Polaris warhead problems and on a small two-stage, partly fusion device. An early plan is listed in Table VI.

On January 9, 1960, Starbird authorized the expenditure of $3.3 million for the construction necessary to meet the readiness dates shown in Table VI, all to be costed in FY 1960. Harold Brown estimated the scientific construction in the tunnels to meet this readiness as costing $625,000 and the long lead-time procurement, cables, etc., as $380,000, for a total of roughly $1,000,000 to come out of FY 1960 funds. The total FY 1960 test funds available were $4,500,000, of which, by this time, $2,600,000 had been committed for Cowboy and Lollipop. In mid-January, ALOO estimated that the scientific construction would be $2,600,000 as opposed to Brown's $625,000, and an extra 20% over this to meet the proposed readiness schedule. Thus, apparently, additional funds would have to be found to meet these readiness dates.

Teller had earlier recommended a change in the containment scaling law from $D=550W^{1/3}$ ft to $D=450W^{1/3}$ ft (where W is the yield in kilotons) which would obviously save money. Starbird agreed with this change on the basis of an assurance that there would be no significant escape of radioactivity. He emphasized that it was not sufficient to simply keep the radioactivity on site.

As noted before, LASL had had into ALOO a request for 1,200-foot holes for some time. However, the pressure on the AEC was not particularly high to accede to this

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5 U.S.C. 552(b)(1)
1.3(a)(4) DOE Exemption 1

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TABLE VI
LIVERMORE READINESS PROGRAM, EARLY 1960

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5 U.S.C. 552 (b) (3), EXEMPTION 3
5 U.S.C. 552 (b) (1), EXEMPTION 1
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request since ALOO commented that the holes could be drilled within three months after word to go back to testing was received. It was LASL's opinion (Bradbury, December 1959) that it would take of the order of three months to get back to testing anyway after the word was given. LASL did have four 500-foot holes that could be used for safety shots or nuclear detonations up to a kiloton. At the end of December 1959 Bradbury had written to Starbird:

> We also propose to request that the AEC now accede to an early request of this laboratory, namely that of digging the 1,200-foot holes to contain shots up to 10 kt, (that you) proceed at such priority as you deem appropriate, and that we be informed of the date when two to four such holes will be available. [redacted] Naturally, we would be more interested in experiments of full yield and perhaps you can assure us that the containment of such shots would not be unduly expensive or delayed in time. Furthermore, if we are correct in our assumption that testing in the atmosphere or in the gravitational field of the earth is too unlikely at this point to warrant any procurement or preparational effort, we would appreciate being so informed. Finally, for the purposes of this year's programmatic statement, we are ignoring the practical possibilities of testing in outer space.

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DOE, Ex. 3

Obviously, LASL was somewhat irritated at the continuing effort to establish some sort of readiness for Livermore without some such corresponding effort for LASL. Thus, in early February 1960, having authorized $3.3 million for the Livermore Succotash readiness program, Starbird authorized $700,000 for possible use by LASL. It was intended that this money be used to advance the LASL readiness to fire in the 500-foot holes already excavated at NTS. In his letter of transmittal to Kenner Hertford, Starbird said:

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~~SECRET~~

> I wish to have you emphasise that this readiness preparation does not constitute any indication, real or implied, that a series will be carried out. It is rather an extension of our efforts aimed at a more advanced readiness state and (it) is desired that no, repeat no, publicity be given to this authorisation for added work at NTS. If questions should arise necessitating an answer, it should be explained as merely a continuation of the work to retain site readiness.

So LASL began another internal go-around in early February, 1960. In general, while Bradbury usually acceded to Starbird's request for some specific list of what should be tested, the internal philosophy was more that "the world was a continually changing thing, our problems were different every week, and, therefore, our path should be to prepare within the funding allowed the most flexible response or capability." Using Bradbury's guideline that testing could not start sooner than three or four months after notification, LASL tried to develop a plan on the assumption that May 1, 1960, would be the earliest possible date of notification, making August 1, 1960 the first date at which testing might actually begin. An additional assumption was that all tests would have to be confined underground.

LASL considerations were in three main classes: future stockpile devices, experiments, and nonstockpile applications.

Future stockpile considerations at LASL included:

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- Transuranic element production.

- Project Orion - General Atomics had been funded for studies of a space nuclear propulsion system that involved throwing nuclear devices out the back end of a ship and detonating them. The resultant particles then hit an ablation plate at the back of the ship to establish propulsion. The characteristics of that ablation plate could be studied in underground tests and General Atomics (Ted Taylor) had sought cooperation from LASL on the subject. It also sought cooperation in building the appropriate devices. LASL was willing to talk, but in general took the attitude "bring money and then we will play."

This was quite a program to tackle with $700,000. H&N estimated that four 1,200-foot holes would take 148 days to construct at a cost of about $170,000 each. In addition, surface facilities and hoist installation would cost on the order of $3,000,000 and would take on the order of nine months procurement and construction. In an attempt to solve the problem, LASL compromised. A plan, transmitted from

Hertford to Starbird on February 25, included a proposal to deepen one of the existing 500 foot holes to 800 feet, and to start drilling two holes to 1,200 feet, with the actual depths to be determined by the constraints of funding. The holes would be drilled within the fiscal year. The plan included ordering two hoists suitable for 1,800-foot depths, and other long lead time items necessary for the surface facilities. On March 9, Starbird approved a variation of this plan as follows: deepen one existing 500 foot hole to 800 feet; knock out the plug at the bottom of a second 500 foot hole and deepen it to 525 feet; start drilling two new holes toward 800 and 1200 feet respectively, and continue as far as time and money permit before the end of the fiscal year; purchase a 35-ton-capacity winch and provide some other long lead time items, all at costs not more than $700,000. By March 10, ALOO, H&N, and REECo were moving rapidly on this plan with orders going out for cabling, racks, hoists, etc. Invitations for drilling bids were to go out on April 6 with the bids due during April and notice to proceed to come on the 29th of April. Projected completion times were then June 3 for the 525-foot hole, June 3 for the first 800-foot hole, June 6 for the second 800-foot hole, and June 30 for the 1,200-foot hole. The job was expected to require three drill rigs of the proper size and capacity.

The system moved with appropriate speed so that the bids for drilling new holes were opened on April 26, 1960.

In preparation for a visit by Chairman McCone to the Laboratories in early May of 1960, both Laboratories prepared statements on their opinions of the situation if testing were to stop permanently, if only underground testing were permitted, or simply if the present situation should go on for some time. In the course of this, Bradbury commented in early March:

> It is my opinion that the probability of nuclear testing in 1960 is so low that the course of the Laboratory should be along the lines "We aren't going to test in 1960; therefore we will start acting as if we weren't and planning for the long range as if we weren't, and keep testing and devices for test very much on the back burner." Then if this assumption turns out at any moment to be wrong, and we are told we can resume testing--well, we fall out and fall in again pointing (and going) in the new direction as fast as possible. Admittedly, on this basis we would not test as soon as we might otherwise after a Presidential directive to resume testing, but we would have a lot more sensible program in the meantime, a lot of people would be driven less rapidly to schizophrenia, and the actual difference in time might not be more than 30 days!

He also commented that it would be difficult to go back to weapons testing because most of the LASL test people had been committed to Rover.

Teller, just slightly earlier (February 2) on the same subject, had commented for Livermore:

> Some very important types of weapons involving really new ideas cannot be developed at all if no nuclear tests are allowed. However, the Lab considers its function to be the most rapid development of nuclear weapons under whatever circumstances may be dictated by national policy.

He reasserted his belief that testing in the near future could only be underground and that the diagnostics would be adequate. He added:

> During the next few years, if such testing resumes, underground sites for testing in the 100 to 200 kiloton range can be constructed and used.

[redacted]

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given for development and preparation of a tunnel.

LASL continued to attempt to catch up with Livermore on underground sampling. Over the early months of 1960, LASL conducted several small high-explosive shots to investigate some of the problems of sampling through pipes into the zero point.

EPG Status, 1960

Joint Task Force

While NTS continued in a state of moderate health because of the funding available for test readiness and the increasing funding for seismic detection experiments and Plowshare, the system concerned with testing at the Eniwetok Proving Ground continued to go downhill. JTF-7 had become a subordinate command of DASA on November 27, 1959, and in order to retain some communication with the rest of the testing community established an Albuquerque office on January 15, 1960, at Sandia Base. Some of the problems associated with putting the Joint Task Force under DASA show up in a memo from Parker, Chief of DASA, to Duncan, Commander, JTF-7:.

> All matters concerning proposed changes to program expenditures and all matters which may detract from your ability to accomplish the JTF-7 mission will be coordinated with this Headquarters.

However, he added:

> You and your staff are authorised the freedom of action necessary in conducting routine day-to-day work in coordination with elements of the military departments and other governmental agencies to accomplish your mission and assure a smooth transition of JTF-7 from a nontesting to a testing period.

And:

> The fact is well recognised in Headquarters, DASA, that JTF-7 may or may not remain under my operational control during the buildup and operational phases of a test period.

With a headquarters complement of 37 officers and 35 enlisted men, JTF-7, during January and February 1960, prepared operations and administrative plans for three types of possible atmospheric operations as follows: high-altitude tests at J.I., open seas tests south of Hawaii, and full-scale tests at Eniwetok Proving Ground.

Operation Switch (the mothballing of EPG to achieve a 12 months readiness status) was essentially complete in late January. JTF-7 rad-safe equipment to the tune of $88,757.42 was transferred to AEC Albuquerque with an understanding that the AEC would be responsible for future procurement of rad-safe equipment as required by JTF-7 in the event of another operation.

In late January James Douglas, Deputy Secretary of Defense, requested that the Chairman of the JCS, DDR&E, and the Military Liaison Committee review their current studies and experimental programs on nuclear weapons effects in light of the guidance:

> If realisation of useful results depends fully on actual tests, underwater or in the atmosphere, these tests should be suspended. If realisation of useful results depends on outer space or underground testing, they should be continued for the present in the planning or preparatory stages with attention given to design and testing of instrumentation. If useful data can be obtained by theoretical computational or simulation methods or by low-order detonations, as defined by the AEC, increased emphasis should be placed on these approaches.

On February 12, 1960, General Duncan recommended to Loper:

> In view of the extremely high cost of maintaining the Eniwetok Proving Ground on a 12-month maintenance standby status, the uncertain future of nuclear testing within the atmosphere and increasing interest in this area shown by other agencies, it is recommended that the requirement for maintaining the EPG at a 12-month basis standby status for nuclear testing be reevaluated at this time.

As a result of these reconsiderations, Admiral Parker (DASA), General Starbird (DMA), and General Duncan (Commander, JTF-7) requested by joint memorandum in mid-March 1960 that Colonel William J. Penley, DASA Army Representative and Chairman of the group; Captain Charles E. Houston, DMA Representative; and Colonel Lawrence M. Watson, JTF Representative, reappraise the requirements for the Eniwetok Proving Ground as well as the readiness status and functions of JTF-7. The Study Group's Report was sent out 12 days later (obviously not reflecting complete coordination with all concerned). It stated that:

> a. Pacific Range, Air Material Command, Tactical Air Command, and Strategic Air Command have all indicated an interest in using all or part of the Eniwetok Proving Ground facilities on a continuing basis for an indefinite period of time.
>
> b. It is politically necessary that there not be complete U.S. withdrawal from the Eniwetok Proving Ground or complete dissolution of the U.S. Nuclear Test Organization.
>
> c. Douglas's memorandum is interpreted as guidance that will continue at least through FY 1961 and states that the 12-month capability to test at Eniwetok Proving Ground no longer needs to be maintained.
>
> d. If testing is resumed, it will probably be underground or in outer space.
>
> e. The Atomic Energy Commission plans to reduce the contractor personnel at the Eniwetok Proving Ground to 300 people from 450 by July 1, 1960, and operate at a total annual cost of about $5,000,000 (as opposed to $8.6 million) without significant change to the state of readiness through FY 1961.
>
> f. There are increasing DOD requirements for effects information which can best be obtained by high-yield shots in the atmosphere or underwater. Testing of high-yield production weapons before stockpile was a requirement which can best be fulfilled by testing in the atmosphere at the EPG.
>
> g. The best place to launch vehicles for outer space nuclear testing . . . would be Johnston Island or EPG.

The group concluded that the Eniwetok Proving Grounds should be reduced to a caretaker status and that JTF-7 Headquarters should be phased down to a planning staff of approximately 36 simultaneously with the phasedown of the EPG. This alternative was judged to have the least effect on present disarmament negotiations and on international, domestic, and psychological factors.

On March 17, 1960, General Duncan, in what was apparently his parting advice to Admiral Parker of DASA, again urged that the 12-month readiness of the EPG be abandoned and went on to suggest that the Eniwetok Proving Ground should be released from even the caretaker status for future testing, which would relieve JTF-7 of any support responsibilities for those facilities. He recommended the continuing phasing down over the next 15 months of the Task Force to the status of a planning staff of DASA, in which circumstance they should be located at Sandia. Strangely, he also suggested that JTF-7 be given the responsibility for providing support for the Nevada Test Site, which was somewhat inconsistent with JTF-7 being nothing but a planning staff. The JTF-7 staff at about the same time made a number of fascinating suggestions with respect to future operations. Among them were: DASA and the Atomic Energy Commission "must" be required to keep JTF-7 fully informed as to what projects and events are being considered as they develop; the Eniwetok Proving Ground Planning Board should be chaired by the Commander of JTF-7; the use of Bikini should be discontinued and any very large shots should be done using the open sea concept; vehicular requirements should be cut drastically; reduce shipments of needless or plush personnel or equipment to the Eniwetok Proving Ground; do not rotate personnel at Eniwetok Proving Ground during an operation. (The author would have disagreed with every one of these recommendations.)

General Duncan, U.S. Army, who had assumed command on November 27, 1959, was replaced by Colonel William S. Hutchinson* on March 23, 1960. JTF-7 now concerned itself with the orderly transition of its assets to other organizations and the problem of leaving some advice for some later Joint Task Force.

On March 30, 1960, the Commission discussed the Air Force (SAC) request to use Eniwetok as a target site for Atlas-Titan (Tick Tock) exercises from Vandenberg and approved the use of the Eniwetok Proving Ground for this exercise on the basis that AEC personnel would be evacuated by the Air Force or provided adequate shelter and the exercise would be announced as an Air Force project clearly not involving the Atomic Energy Commission. In late April and early May, the Chairman of the AEC (McCone) and the Secretary of Defense (Gates) agreed that the Eniwetok Proving Ground should not be retained as a nuclear test facility except on a caretaker basis and that it should be transferred to the Pacific Missile Range by July 1, 1960.

On May 24, 1960, the Task Force initiated a project for turning over the EPG and the liaison offices to PMR. It seemed wise to consider the use of H&N employees to provide certain services there after the transfer in order to maintain an AEC presence in the area. SAC conducted their first exercise in June. H&N support for the exercise suffered because of their expectation of significant reduction in staff by July 1, when the atoll would be transferred to PMR. By June 10, 1960, Charles Kelley, Vice-President of H&N, and Ray Emens, the Honolulu representative of the AEC, were far along in their discussions of the Pacific Missile Range takeover. As of July 1, 1960, the LCMs and LCUs assigned to the AEC at the Eniwetok Proving Ground were transferred to PMR; the Air Force transferred one L20 aircraft and spare parts to PMR; enforcement of plant and animal quarantine laws was assumed by PMR; and PMR took on the job of submitting requirements to MATS for transportation. In essence, the transfer of the Eniwetok Proving Ground from the AEC to PMR became complete. The responsibility for the continuation of leases on the outlying islands around the Eniwetok Proving Ground (used for weather stations and radiation monitoring) was transferred from the AEC to DASA on this date. Coincident with the above actions, Task Force Headquarters personnel were reduced to 28 officers and 41 enlisted men. The JTF-7 meteorological effort was transferred to the University of Hawaii. The effort during this period of time was known to the Task Force Headquarters as Operation Phasedown.

On June 15, 1960, the JCS recommended to the Secretary of Defense that JTF-7 be reduced to a planning group of 10 personnel within the DASA organization.

*Colonel Hutchinson wrote to Colonel Thomas L. Mann on May 19, 1960: "General Harrison has notified you of your selection for assignment as Commander of JTF-7. I'm sure that you will find this somewhat puzzling; however, be assured that this is a highly desirable assignment upon which you are to be congratulated." The reply came from Colonel Mann (in Iran at the time): "I am quite puzzled and am looking forward to the assignment with anticipation mixed with quite a bit of reservation."

Task Groups

As previously related, the scientific task group 7.1, had been wiped out in late 1959. The other task groups were soon to have their own problems.

Task Group 7.2 (Army) apparently had a very small flurry in early January when there was a proposal to reassign the Army personnel at the Nevada Test Site to 7.2 which would now be headquartered in Arlington. However, consistent with previous plans, on January 17, 1960, Task Group 7.2 left the Eniwetok Proving Ground and was transferred without personnel or equipment to Arlington Hall Station.

The Navy Task Group (TG 7.3) had found other work to do unrelated to test readiness in addition to their normal function. They supported a series of tests off the Florida coast in February and March of 1960 which evaluated various methods of locating and recovering pods for DASA. These tests investigated various configurations for recovering pods which might be ejected from missiles at high altitudes during Operation Willow. They continued to study precise positioning of targets using deep sea mooring. (Admiral D.M. Tyree was still Commander of 7.3, continuing from Operation Hardtack.) From March to May, the Task Group was involved in a series of mine damage tests conducted near Puerto Rico.

Apparently because most of 7.3's work at this time was actually for DASA, the Task Group was reassigned to Headquarters, DASA, on March 22, 1960. (That was one way of relieving the Commander of JTF-7 of his responsibilities.)

Task Group 7.4 (Air Force), whose capability was really the 4950th at Albuquerque, continued its ups and downs. In December 1959, General McCorkle of the Air Force Special Weapons Center in Albuquerque was still resisting the JCS decision to form a permanent Task Group 7.4 and suggested to General Canterbury of the Research and Development Command that the most appropriate move was simply to designate Headquarters 4950th as also Headquarters Task Group 7.4, continuing its AFSWP responsibilities. On January 13, 1960, General Duncan (JTF-7) agreed with this proposal, requesting specifically that the 4950th be responsible for exercising operational control over resident U.S. Air Force elements at Eniwetok Proving Ground during the periods between tests and that the responsiveness to the Task Force be determined by mutual agreement from time to time between the Commander of JTF-7 and the Commander of AFSWC. A charter was written to define this action, to become effective January 15, 1960. However, by June 20, 1960, the Headquarters of the 4950th had not yet received final notification on this move. In October of 1960, AFSWC was informed that Headquarters, U.S. Air Force, had not acted on the proposed charter and did not propose any further action until revised plans and concepts on overseas testing were established. As far as the author is aware, the permanent Task Group 7.4 was never activated.

Even without the specific charter of 7.4, the 4950th continued its vigorous support of the nuclear weapons program. However, Colonel Byrne's spirits probably dropped when he was informed in the last week of January that Willow would probably be cancelled. On April 12, 1960, McCorkle recommended to Schriever that the 4950th be relieved of its responsibility for maintaining a readiness to resume air support of nuclear testing within 12 months and, in fact, that they simply be deactivated. The 4950th Headquarters was reduced from 25 officers, 45 airmen, and 2 civilians in September of 1958 to 12 officers, 26 airmen, and 2 civilians in April 1960. By July 1960, the Commander, Keith Byrne, was sufficiently discouraged to recommend to AFSWC that the 4950th be disbanded and the 4926th be transferred to AFTAC. Headquarters was further reduced to 7 officers, 6 airmen, and 3 civilians on July 28, 1960.

Apparently, the 4926th Sampling Squadron was to be left alone. In mid-February of 1960, it was noted that of the six B-57D aircraft modified for high-altitude sampling, one had been destroyed in a crash, three were assigned to Air Defense

Command, and the other two were no longer in the active Air Force inventory. Because the B-57s were getting harder to maintain, the 4926th, in conjunction with the Laboratories, studied the question of a possible replacement. They concluded that the McDonnell F4H was the most suitable aircraft, with the A3J-1 the second choice. However, the 4926th Sampling Squadron managed to maintain its strength even though by mid-1960 they only had eight sampling configured aircraft. Their function continued to be valuable for several reasons, including sampling of the Rover and Pluto effluents in Nevada and their increasing assistance to the AFTAC. [redacted]

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The 4951st Support Squadron at Eniwetok was caught up in Operation Phasedown and was reduced in personnel and relieved of its job at Eniwetok. By July 1960, the air control function had been lost because of transfer of personnel. (The equipment was put in storage.)

Johnston Island

The test system backed out of Johnston Island in early 1959 on the basis that the Army intended to use the island after installing a Redstone or Jupiter launch capability there. This assumption proved to be incorrect. The Army intended to launch Jupiters (IRBMs) from Johnston Island as targets for the prototype Nike-Zeus system installed at Kwajalein. [redacted] During 1959, while negotiations were going on between the Army and the Air Force concerning the possible transfer of the island to the Army, plans were made to enlarge Johnston Island by 23 acres using fill dredged from the ocean bottom. A contract for that work was awarded July 9, 1959, with the completion scheduled for February 1, 1960. The coral fill construction program was completed in June of 1960, resulting in an addition of about 25 acres to the north side of the island. In December of 1959, the Secretary of Defense granted permission for the installation of a LORAN (long-range navigation) station on Sand Island with the provision that it operate on a noninterference basis with the proposed Nike-Zeus program. Since Sand Island was a Department of Interior bird refuge the occupants were required to observe bird refuge regulations. [redacted] Thus, the intercept exercise became of somewhat lower priority and was eventually cancelled, and the transfer to the Army that was to take place in December of 1959 did not take place. By August of 1960, Air Force retention of Johnston Island seemed assured and a program of rehabilitation of urgently required facilities and equipment was begun in early 1961. Such things as airfield pavement repair, emergency runway lighting, repair of the distillation system, and installation of new equipment were accomplished. Island population during this period of time was approximately 150 people. The launchers upon which the Willow planners had depended were never constructed.

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WET/AFSWC, 1960

LRL began to phase out of the Marshmallow (Jericho) program the first of the year, and that effects shot became the responsibility of Field Command, DASA, on April 1, 1960, at which time it was intended that it be brought by October to a 12 months readiness and simply kept in that status from then on. Work continued on the

Vela Uniform high-explosive shots. Colonel Leo Kiley, who had become head of Weapons Effects Test on February 14, 1960, thought the future looked sufficiently discouraging that he recommended, on April 22, substantial reduction of his group. The number of personnel assigned to the Weapons Effects Test (WET) part of Field Command dropped from 113 in January to 97 at the end of June 1960.

Work began to increase for WET in the second half of 1960, largely as a result of increased Vela Uniform effort, in which they had the responsibility for measuring the close-in seismic signal and ground motion. Work continued on Marshmallow so that by the end of 1960, a 12 months readiness status was expected to be reached by May 1961. As the work load grew the number of personnel climbed back up to 104 people, and Jim Barton became the head of the Vela Uniform office in WET. FY 1961 Vela Uniform funding was 4.3 million dollars, of which 2.1 million had been committed halfway through the year. WET also became responsible for the conduct and control of the Vela Uniform high-explosive shots outside of the NTS. On August 21, 1960, Major General H.C. Donnelly (USAF) assumed command of Field Command, DASA, from Major General Louis T. Heath (USA).

The Air Force Special Weapons Center competency in high-altitude phenomenology began to grow appreciably in this period. Their efforts along this line had begun with the instrumented Jason sounding rockets used to measure the effects of the three Argus detonations in 1958. During the moratorium their efforts continued, using Javelin rockets and instrumented pods on Atlas ICBMs. Their Journeyman sounding rocket capability had been developed to a capability of taking space probes higher than 20,000 miles. In August of 1960, AFSWC suggested to Bradbury, following discussions between Herman Hoerlin of LASL and W.D. Henderson of AFSWC, that the two organizations co-operate in this field. Colonel Jones of AFSWC had supported for some time a contractual effort to describe the time and space history of bomb debris from high-altitude detonations and now proposed to analyze the previous assumptions to select that model which most nearly described the actual physical condition, and to predict the motion of bomb debris for explosions at different altitudes, locations, and yields. Bradbury replied to Colonel Jones that the LASL interest was to "look at the early time history of the expansion of a bomb plasma in space" and that LASL would address the physics of the early phase in detail fully before considering, if at all, the later times. However, he agreed to keep AFSWC informed of LASL's progress. By November, AFSWC was developing, specifically for Vela Hotel, a low-cost sounding rocket, which resulted in the solid propellant vehicle, Blue Scout Junior.

Vela Hotel, Early 1960

In early 1960 the AEC, NASA, and the Air Force (Air Research and Development Command-ARDC) were, at the request of ARPA, preparing a "Development and Funding Plan for High-Altitude Vela." Sandia and LASL, via the "Buzzer" committee, developed the AEC input to that plan. The plan, published by ARDC as "Project Vela Hotel, ARPA Order 102-60" was reviewed by the high-alitutde detection subgroup (Panofsky Panel) of the ARPA ad hoc group on the Detection of Nuclear Detonations (chaired by Richard Latter).

The plan, as submitted in April 1960, called for putting three satellites on station within four years at a total cost of a hundred million dollars. The LASL budget would be a million dollars per year and the Sandia budget five million dollars per year [redacted] Detection of the various weapon outputs was considered wise as a help in identification in any case and in particular in case an evader tried to suppress the radiation from the bomb.

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In order to complete and test the design of the satellite components a large number of small rocket flights would be necessary to furnish test beds for prototype instrumentation. Indeed, some such flights had already taken place from Canaveral and the Tonopah test range, and more were planned from these launch sites and others, including Point Arguello, Wallops Island, and Fort Churchill. Missiles included Atlas, Journeyman B, and others.

The ARPA subpanel recommended that the proposed plan be followed, but requested investigation of the possibility of an interim capability that could be carried out in 18 months, but probably with reduced sensitivity from that planned later.

In response to that request LASL and Sandia produced a "Proposal for Interim Capability in Outer Space, ICOS" on June 10. The proposal was for a one satellite capability, with sensitivity reduced a factor of ten below that previously suggested, using only presently proven components and techniques, and to be deployed in 18 months.

The ARPA subpanel reviewed this proposal in mid-June 1960 and recommended that the high-altitude detection program be expanded, that a capability be deployed as soon as possible, and that the Atlas/Agena B be used as the lift vehicle. They then endorsed the technical approach proposed in the two studies mentioned above.

Vela Sierra, Early 1960

In a February 9, 1960, message to Hertford, Bradbury, and Molnar, Starbird discussed an upcoming meeting of the "Principals" in the next 10 days to discuss the future of the detection system, the research and development programs, and the division of responsibility. Starbird saw the area of high-altitude detection by ground stations as being entirely LASL's (within the AEC Laboratory structure) responsibility, with the direct optical and fluorescence systems as part of the first step. LASL, with DMA concurrence, arranged that EG&G build the LASL-designed fluorescence system prototype and told EG&G that they might be asked later to build the direct optical system. During March, EG&G was funded. Target dates for the fluorescence system were established as follows: approval of the proposed system design on June 1; prototype delivery to LASL on August 1; lightning discrimination tests completed at LASL by September 15; auroral background tests in October and November; and system evaluation completed by January 15, 1961. Furthermore, a prototype of the direct optical system, to observe visible light from the expanding bomb debris, was to follow the fluorescence system by two months.

Discussions between Carl Walske at Geneva, and Don Westervelt* of LASL, in late May and early June, brought out the possible value of pinpointing the direction of a clandestine detonation as an aid to the fluorescence system. Walske felt that no one detection system would give the U.S. enough information to challenge the Russians with an accusation of violation, and that corroboration by another method was vital.

Plowshare, 1960

Proponents continued to develop plans for the various Plowshare prospects during 1960, but the hope of conducting actual detonations during the moratorium did not come to fruition, and the program had budget problems because of the growing needs of the Vela Uniform program. Some of the specific actions are related below.

*Westervelt was the focal point for the atmospheric fluorescence system design.

Gnome

Project Gnome was discussed in several meetings of the Commission and the Plowshare Advisory Committee during January and February 1960. The January 12 Commission meeting, attended by ALOO environmental hazard experts, included discussions of potential contamination of the aquifer and possible triggering of earthquakes. The recommendation of the Plowshare Advisory Committee was accepted by the Commission at its January 22 meeting, and led to the decision to proceed with site preparation and construction which would be disclosed publicly at the time bid proposals were requested. In their February 4 meeting, the Commission summarized the objectives of Gnome as heat production for power generation, investigation of the feasibility of recovering beneficial bomb radioactive isotopes, and extending knowledge about characteristics of an underground explosion in a medium (natural salt) having physical properties in marked contrast to the only medium (volcanic tuff) for which such information was presently available.

Indicative of LRL efforts to pursue Plowshare projects, Brown noted in his status report of July 15, 1960, that Gnome was the only nuclear experiment for which construction authorization then existed and that funding reductions had caused discontinuation of high-explosive studies and reduction in the level of certain feasibility studies. Livermore was determined "to make Gnome as successful an experiment as possible, believing that a single successful Plowshare demonstration may very well affect decisions on the budget and authorization."

By June 9, 1960, the Gnome plan had been expanded to include basic physics measurements as well as the earlier planned experiments on heat and isotope production and underground nuclear explosion phenomenology. Basic physics experiments included four neutron experiments that could not be done in the laboratory, three of which utilized moderated neutrons to study fission and capture reactions and resonance parameters for certain heavy nuclei, and the fourth using fast neutrons to study direct interactions in inelastic scattering off carbon.

Noting the stagnation of the overall Plowshare program as a result of negative public reactions, the Plowshare Advisory Committee, at their meeting of October 19 and 20, 1960, strongly urged that the LRL plan for Gnome be executed as soon as possible.

The AEC's 1960 annual report noted that full preparation, excluding device emplacement, was authorized on March 16 and the prime contract was awarded on June 9.

Oilsands

The Oilsands project (to recover oil from tar sands in the Athabasca region of Alberta) was discussed in the January 22 Commission meeting, which was attended by Gerry Johnson, Philip Farley, and representatives of the Richfield Oil Company. Richfield representatives expressed their opinion that an economical return from initial tests would be more likely if the yield of the nuclear device were 100 kt instead of the planned 9 kt. They also stated it was their belief that the Canadian government was waiting for the U.S. to go ahead with Gnome before granting approval for the Oilsands experiment.

A related LRL effort (Pinot) conducted in Rifle, Colorado in August 1960 was a small-scale, high-explosive experiment intended to measure the migration of gaseous products along bedding planes in oil shale.

Chariot-Ditchdigger-Panama Canal

During 1960 the fates of the Chariot, Ditchdigger, and trans-isthmus projects were tightly bound by the problem of fallout from cratering detonations. Chariot, the harbor project near Cape Thompson, Alaska, was to be carried out as soon as possible, depending upon bioenvironmental studies. While the purpose of the experiment was to produce a harbor, the fallout and cratering data obtained would be used to guide planning for the proposed new trans-isthmus canal. However, it was clear that the canal would have to be done with clean explosives, producing as little fission as possible. The main candidate for that explosive was the "Ditchdigger" device. That device, however, would not be available for use in the Chariot experiment, which would use a normal fission bomb.

On January 17, 1960, a Mr. Rutledge, the only Plowshare Advisory Committee member who had not previously been associated with any atomic energy work, wrote a letter to Spofford English stating:

> To my mind, the future of the whole Plowshare program is endangered if Chariot is permitted to go ahead without having a Ditchdigger in progress and without being able to tell the public that Chariot is entirely experimental and that it is the last time that conventional atomic bombs will be used for such a purpose.

The Committee itself had recommended at the January 14-15 meeting that:

> ...each Plowshare detonation be highly instrumented; the Ditchdigger program be initiated as soon as possible in order to demonstrate Ditchdigger principles by the time the Chariot detonations are carried out; the bioenvironmental survey work for Project Chariot be continued at the present level but not further expanded.

The Commission reviewed this recommendation at their January 22, 1960, meeting and concluded that the AEC could proceed with Chariot only after resolving problems with the White House, Congress, State Department, and other federal agencies, but that it would be all right to go ahead with the ecological survey as long as there was no other shot preparation. They approved the survey and developmental studies of Ditchdigger (at LRL). However, at their February 4 meeting they expressed their feeling that a Ditchdigger experiment, wherein the tunnel and instrumentation configuration could be open for inspection, would not be possible in the near future.

On March 2, addressing the Chariot project, the Commission reiterated its decision to continue to authorize the bioenvironmental surveys, but approved a target date for the firing in spring of 1962 which obviated the need for any construction work in the summer of 1960. Furthermore, they requested that recommendations be furnished to them by October of 1960 as to whether to proceed then with planning for the experiment in the spring of 1962. Approximately 72 people were involved in Alaska in the work on this project through 1960.

Teller briefed the President and his cabinet on April 29 and followed up with a letter to Eisenhower concerning the trans-isthmus canal. He stated that this was the most ambitious of the Plowshare projects in view, and that it appeared to Livermore that its technical feasibility was assured. He then presented to the President two preparatory steps which would lead up to the development of a trans-isthmian canal in an orderly way. The first of these was the development of nuclear explosives such as envisaged in the Livermore Ditchdigger proposal. He pointed out that combining reduced fission designs with deep burial of the explosive would reduce radioactivity released to the atmosphere by a factor of 1,000. Use of the Ditchdigger concept would considerably decrease the number of persons having to be evacuated for safety reasons. Thus, emotional objections would be greatly decreased although, as Teller pointed out, "None of these people need worry about radioactivity at all." The second developmental step would be a trial run on a reasonably large scale. Teller felt that this should be in the U.S. in an area with little population and that the harbor project in northern Alaska (Chariot) would be suitable preparation.

Not everyone was as optimistic. Following the Plowshare Advisory Committee meeting of May 25 and 26, another one of its members, Willard Bascom, came out against Project Chariot, noting that since future ditches would be dug by another method, the Project would demonstrate very little. He recommended immediate abandonment of the project and transfer of the funds to something of greater promise.

By November of 1960 the plan for Project Chariot had been modified a couple of times. The present version would create a channel to the ocean and a turning basin by detonating one 200-kiloton and four 20-kiloton nuclear devices. All that had been accomplished to date, other than one small (256-pound high explosive) detonation, were studies on the environmental effects of the program execution. Such studies were to continue until final recommendations for proceeding with the project, expected about March 1961. No funding was presently programmed for the project beyond that date. Based on possible firing dates for 1962 or 1963, the overall project could cost $12,000,000 or $13,000,000, of which $2,500,000 had been spent to date.

On December 2, the Commission considered the Plowshare budget and future authorizations in light of a recommendation from the Bureau of the Budget for a $7,500,000 reduction for FY 1962. The Commission felt that they could accept $6,500,000 of this reduction, given its restoration in the event of a test resumption. However, they felt that $1,000,000 was required to permit proceeding with the necessary Chariot related high-explosive experiments. Starbird felt that the total $7,500,000 reduction would require eliminating the Ditchdigger and Chariot projects. The Commission agreed to request an additional $500,000 from the Bureau of the Budget to keep the project going.

High-Explosive Cratering Experiments

During 1960, Sandia carried out, at the NTS, a major portion of the Plowshare high-explosive cratering experiments designed to establish scaling laws in different rock and soil media. Project Buckboard, carried out in the summer of 1960, involved a number of high-explosive shots (from 1,000 to 40,000 pounds) in holes of various diameters and depths in basalt. Project Toboggan, also conducted in the summer on Yucca Lake, consisted of linear cratering experiments with high explosives (up to 8,500 pounds) in alluvium. Project Scooter, a 500-ton high-explosive detonation in a sphere 125 feet below ground at Yucca Flat, was attempted and misfired on July 14. (Rumor had it that sugar detonators instead of the real thing had been installed.) Representatives of the contractors, as well as both Sandia and Livermore, briefed the Commission on August 30, 1960, on details of reentry of Project Scooter. The shot was successfully carried out in October of 1960.

Vela Uniform, 1960

The Black Box Problem

During 1960, Black Box devices were discussed in connection with seismic detection, as well as with Plowshare. The discussions related to detection started from the U.S. idea to furnish devices for Vela Uniform experiments in such a manner as to convince the Russians that we were not conducting weapons tests, and at the same time satisfy the U.S. law that precludes disclosure of device design to foreign nations.

The need for such an idea arose from the uncertainty in the seismic detection threshold of the Conference of Experts Geneva network which had been introduced because of the results of the analysis of the Hardtack Phase II data and the introduction of the Latter big hole theory in late 1958 and early 1959. On February 11, 1960, as part of the new U.S. proposal for a treaty banning detonations in the atmosphere and underground above the seismic threshold of 4.75, Ambassador Wadsworth commented that the United States had already "embarked on a major experimental program aimed at the discovery and development of improved means of seismic detection and identification." Among the methods and techniques used in this program, there might be nuclear explosions, if they were required, and as part of the U.S. proposal, he invited the U.K. and the Soviet Union to join the United States in instituting a program of joint research. On February 16, Tsarapkin commented that such a research program could begin immediately after signing of the treaty, and added that nuclear devices would not be required in the program, that chemical exposives would be sufficient. Repeating the U.S. suggestion on March 29, in reporting on their meeting at Camp David, President Eisenhower and Prime Minister Macmillan stated that they had agreed to "invite the Soviet government to join at once with their two governments in making arrangements for a coordinated research program and putting it into operation." On April 9, in proposing a conference on the technical aspects of the proposed research program, to be called the Seismic Research Program Advisory Group, Ambassador Wadsworth again discussed the type of "coordinated research program" that the United States felt should be carried out. On May 3, in accepting the new Western position, the U.S.S.R. agreed that as part of the planned research, there might be a "strictly limited number of joint underground nuclear explosions." During the discussions of the Seismic Research Program Advisory Group which began on May 11, the Russian delegate, Mr. Riznichenko, commented on May 14, "It seems obvious to us that at the present time, a certain number of coordinated nuclear explosions of definite magnitude or energy will have to be carried out by us." This led the US momentarily to believe that some of the experimental explosions might be in the U.S.S.R. However, at the next meeting on May 16, Riznichenko announced that there would be no nuclear explosions in the U.S.S.R. and that the references in the Soviet paper were to nuclear explosions envisaged in the American program. Futhermore, at the diplomatic conference on May 27, Tsarapkin stated that the Soviet Union had never believed it was necessary to carry out underground tests for research purposes, and since it was the United States that insisted on the explosions, they should be carried out by the United States on its own territory. He also demanded that Soviet scientists participate fully in carrying out underground explosions on the territory of the Western powers, and asked what safeguards would be used to make sure the underground nuclear explosions were not used to improve weapons. Ambassador Wadsworth, in his reply six days later, stated:

> First, the party shall detonate only nuclear devices of proven design in its experiments. Secondly, the devices used in these experiments shall be taken from a special depository of previously deposited devices established by the party within its territory. We consider that such prior deposits should take place within the shortest possible time and thus propose specifically August 15 of this year as the time limit. The devices suitably packaged shall be under the constant surveillance of representatives of the other parties, or, if preferable, of an international group. The devices may not be altered and devices once withdrawn from the depository may not be redeposited. A device removed from the depository in order to be detonated shall remain under surveillance until detonated. Third, the representatives of the other parties or of the international group responsible for surveillance shall be permitted to observe all aspects of the detonation and its instrumentation except for the interior of the package. Fourth, no diagnostic instrumentation will be used in the near vicinity of the device except for specific yield measurements. Such yield measurements as are made under this provision will, of course, be under the surveillance of the other parties or the international group as the case may be and all the information thus obtained will be available to them.

Ambassador Wadsworth commented that these safeguards had been adapted from those which the United States had previously proposed in connection with the peaceful nuclear detonations. On January 30, 1959, Wadsworth had proposed that nuclear devices to be used for peaceful uses explosions would be placed in a depository on or before the date of entry into force of the treaty and would be kept under surveillance from then on until used. Interior inspection of the device would not be permitted. This was the so-called "Black Box" proposal. Tsarapkin, on February 23, 1959, had rejected this "Peaceful Uses Black Box" proposal, charging that it would permit the stockpiling of new nuclear weapons and their testing, and introduced an article proposing (a) prior submission to the original parties with complete description and the blueprints of the construction of the device to be exploded, and (b) inspection of the internal and external construction of the device.

On June 15, 1960, Tsarapkin rejected Wadsworth's "Vela Uniform Black Box" proposal as a "fictitious" safeguard, repeating that Soviet scientists must participate in any underground explosion program and that there must be effective control to ensure that such explosions were not used for nuclear weapons development. He insisted on the following conditions:

a. A full description and blueprints of the structure of the device to be exploded must be made available beforehand to the other participants in the program and they must be allowed to inspect the internal and external structure of the device.
b. The representatives of all participants in this program must be present at the place of assembly and explosion of the device.
c. The instruments and measurements apparatus shall be installed by all the participants in the program.
d. All data obtained as a result of the explosion shall be made available to all participants in the program.

After due consideration by the United States, Ambassador Wadsworth suggested on July 12 that the three powers pool a number of nuclear devices of militarily outdated design. All three parties could then examine the internal design of the devices which would be kept under joint technical surveillance until detonated. Thus, the U.S. was asking the Soviet Union to supply some of the nuclear devices, even if the explosions were to take place in the United States. He also stated that if the Soviet Union agreed to this pooling proposal, the President was prepared to seek Congressional authorization in August to permit internal examination of U.S. nuclear devices to be used in the seismic research program. On August 2, Tsarapkin noted this move as a favorable sign and stated that the United States and United Kingdom could set up a pool of such devices if they wished, but the Soviet Union refused to be involved as a supplier of nuclear weapons to the United States for research that it considered unnecessary. He reiterated the Soviet Union demand for conditions such that:

> When the United States carries out nuclear explosions for purposes of research, no loophole will be left, either directly or indirectly, for improving existing types of nuclear weapons or for testing new types.

All of this had not taken place without active participation by the weapons laboratories and testing community and without some effect on their budget. On February 24, 1960, Kistiakowsky, the President's Scientific Advisor, had written to the Chairman of the Atomic Energy Commission:

> The President has asked me to inform you and Secretary Gates that he considers a vigorous and continuing research and development program on the detection of underground and high-altitude explosions to be a matter of high priority and that he hopes the DOD and AEC will find it possible to finance this program for FY 1961 within their existing budgets. I am forwarding a copy of this letter to Secretary Herter.

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The February 11 invitation for a joint program obviously triggered off appreciable discussion in the Laboratories and the AEC on how such a joint program should be carried out and what the problems would be. It was recognized early on, as a result of the Plowshare discussions the previous year, that device considerations would be a serious problem. The announcement on April 9 of the intent to hold a conference on underground detection in early May increased the pace of the discussions. It had become clear early on that we would not propose to use our most modern devices in a situation in which the Russians could legally obtain the diagnostic information on those devices, even if they were not privy to the internal designs, because the diagnostics in themselves, especially radiochemistry or reaction time, could reveal some of those design features, for example, boosting. Thus, the thoughts clearly centered around old devices from which not much could be learned. By late April 1960, the concept of putting away a stockpile of devices for use in the then planned seismic detection program was already being discussed seriously and the search was on for an old device which could be stockpiled in sufficient yield variation to satisfy the requirements of the program. Starbird, on April 26, made the point to the Laboratories that it was not necessary to state that either the weapons are obsolete or that all versions had been previously tested, but we should be able to state that they were standard, older-type weapons. The Mark VI, 39 inch diameter, and the Mark VII, 27 inch diameter, were discussed, with the Mark VII being more desirable because of the requirement to lower the device in its container down a 36-inch hole*. Such careful wording would, in his opinion, allow prompt production and stockpiling of the devices in whatever yield was desired, even though the yield might not be a stockpile number. Livermore was already procuring containers for the device. On the same date, Starbird asked the Laboratories for advice on how the devices might best be stockpiled, possible methods of emplacement, etc., and requested that the Labs work together in preparing answers. He proposed that the stockpile be at [redacted] Since the Mark VII was a Los Alamos device, Los Alamos was asked to comment on the accuracy of the yield prediction for variations not previously tested. Jane Hall, on April 26, stated that the predicted yield for untested versions of the Mark VII should be good to plus or minus 10%. She added that the uncertainty of yield determination using radiochemical methods for underground shots would be 15% or 20%. At the same time Starbird furnished to the Laboratories a list of the possible energy releases required for the proposed Vela underground shots. On April 27, ALOO recommended against storing the devices at [redacted] since it was a normal stockpile site, suggesting instead, military ordnance depots such as Wingate, New Mexico, etc.

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On April 28, 1960, Starbird recommended a U.S. position to the Commission. Part of that recommendation was that no diagnostic instrumentation be allowed but acquisition of radiochemical samples would be allowed. If these provisions were acceptable to the Soviets he would prefer the Mark VII as the explosive. The devices would be placed in Black Boxes and no internal inspection allowed. If none of this was acceptable to the Russians, then a proposal in which we actually allow inspection of some of the older devices could be made as long as there was a quid pro quo from the Russian side. At the same meeting, Colonel Sherrill stated for Loper a different position, that of simply going ahead with the Vela underground program unilaterally, offering the Russians the freedom to monitor it if they liked, and simply stating that we were not doing it for weapons development.

Teller was not enthused about the Black Box proposal, feeling that if the Russians did the same thing, they would cheat and make them into weapons tests, so he, too, proposed that we go ahead unilaterally with our own Vela program.

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*The Black Box had to be watertight and capable of standing fairly large hydrostatic pressure since some of the possible seismic improvement shots involved emplacement below the water table in uncased holes.

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The Commission, on May 3, agreed with General Starbird that the storage of some 25 devices seemed reasonable.

The Principals approved the Black Box concept as the U.S. position at Geneva on May 10.

On May 17, Starbird instructed Hertford of ALOO to prepare to stockpile some 25 devices[redacted] in order to be prepared for at least 12 different shots. The devices should be designed to fit into a 36-inch diameter hole, and the Black Boxes should be in storage by August 1, 1960.

On May 18, the Commission approved (subject to the condition that the Russians accepted the whole Black Box proposal) declassification of the debris of the Mark VII devices used in the seismic improvement program or in Project Plowshare. However, at the same time, they noted that obtaining samples of Russian debris would not serve as a basis to determine whether or not the Russians might be carrying on a weapons test program in connection with the seismic improvement program or Plowshare program.

In parallel with these actions came the debate on the question of how to determine the actual yield of the detonations. There were several possibilities, all of which were discussed at one time or another. Radiochemical analysis of samples obtained by core drilling into the detonation region would give a good measurement of yield if the amount of active material in the device were known. Radiochemistry could also be used to compare two devices that were stated to be the same. To assist in the radiochemical determination of yield, it was also suggested that the other countries could put tracers next to the nuclear explosive to help determine the fraction of the bomb materials that might be collected in a given sample. The measurement of alpha (reaction history) on a single-stage unboosted device could be used to determine that the device operated as previously predicted, but would not give the yield directly. Measurement of time of arrival of the shock near the detonation would, in principle, give the yield independent of any details of the design of the device, but there was no appreciable experience underground with the technique. It was expected that the uncertainty of the measurement would be about 50%. One could, in principle, determine the yield from a measurement of the radiation temperature reached in a bottle surrounding the device. However, this was also an untried method and its accuracy was unknown. It was fairly clear, however, that the technique could be used to compare two similar shots. Livermore favored still another method, that of the measurement of the time of transit of radiation through a polyethylene block next to the bomb, and began construction of a polyethylene box that would allow application of that technique.

On May 25, 1960, Bradbury told Starbird that LASL felt the best method for determining yield was through a simple measurement of peak alpha which could be made by Sandia or EG&G.

> The LASL strongly recommends that such a measurement be included and openly identified as completely uninterpretable without a knowledge of the bomb being observed. It actually yields far less information than do the radiochemical samples. It is LASL's opinion that such a measurement could be regarded as an obvious and elementary method of yield determination necessary to the program to avoid ambiguous results in case of weapon malbehavior for some reason. The LASL recommends against attempts to determine yield by radiochemical methods.

He went on to state that LASL would expect to be the agency primarily responsible for theoretical predictions and yield determination for those bombs for which LASL provided the active material capsules.

At this point, on May 27, Tsarapkin made his safeguard statement at Geneva and on June 2, Wadsworth answered with the Black Box proposal.

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Meanwhile, on May 23, Starbird had authorized expenditures of $200,000 for production of the required capsules and noted ongoing negotiations with the Department of Defense to withdraw the necessary weapons from stockpile. Procurement and construction of the Black Boxes was to be a Sandia job. [redacted] The schedule was still to have devices at the storage location not later than August 1. The tentative firing dates for the first two shots of the Vela Uniform program were September 1 and October 15, 1960. [redacted] By June 1, the storage of devices was known as the Whirlaway Program. On June 3, 1960, Sandia estimated for Starbird a Sandia cost for this program of $133,000 for FY 1960, $1,000,000 for FY 1961, etc. By June 8, Bradbury had thought a little further on the yield question and suggested to Starbird that LASL be responsible for giving the expected yield, but that Livermore be responsible for whatever yield measurements were made, and for issuing the experimental value of the yield. He further stated:

> This proposal is made for a variety of reasons, one of which appears to be that there are already too many cooks stirring this broth with conflicting opinions and advice, another is that logically the organization which states the a priori expected might be accused of bias in experimental measurements thereof, and a third is that with practical certainty, differing values of the experimentally observed yield will be obtained by different organizations, both domestic and foreign, and there is no need to add a third party to the argument.

However, he does state that LASL would desire to have some of the radiochemical debris for their own study and analysis and be involved in any plans for hydrodynamic measurements. In a June 9 letter to Starbird, Bradbury was a little more frank.

> It is my own opinion that the inclusion of tracers (supplied by the U.S.S.R.) and indeed, the whole radiochemical measurement and distribution of samples is a mistake and will open up a Pandora's Box of bitter argument.

He pointed out that the Russians could easily doctor the tracers and, thus, make the samples look larger and the bomb yield look smaller. They could also select the right samples to give the results they wish and accuse the U.S. of doing the same thing and, hence, engender a yield argument between the two nations. He remained of the opinion that the measurement of alpha was the only way to get a quick, accurate, internal estimate of the experimental yield. He went on:

> I am reminded more and more of the Hardtack proposal for a "clean" bomb demonstration for the Russians. This eventually fell through for the same sort of reasons which will haunt Project Whirlaway: one cannot objectively and unassailably demonstrate good faith within the framework of the legal restrictions of the Atomic Energy Act! If this were my job, I would take these "nominal" bombs, shoot them off, measure the seismic disturbance at the distance of interest, and do nothing else! Everything else we do is (or can be construed to be) learning something about bombs or their effects unrelated to Whirlaway.

He ended the letter by saying that these were his "last remarks on this unpleasant subject."

On June 10, Harold Brown of Livermore stated that Livermore was quite willing to accept the responsibility of making yield measurements on Whirlaway. They would be happy to provide samples of the debris to LASL and stated:

> LRL fully expects to view the LASL calculated yield values as the correct ones and the "experimental" value which we issue will be for the purpose of satisfying the international coordination requirements.

The feeling that the Russians might not accept the Black Box concept apparently grew in Washington in the first week of June and on June 9, Starbird informed the

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Laboratories:

> **High-level decision made here today that we give consideration on an urgent basis to the feasibility of using older gun-type devices for the seismic improvement program. This, of course, to enable opening of devices for U.S.S.R. inspection.**

He requested information on this possibility by June 13. The Laboratories replied on June 13, with Harold Brown (Livermore) commenting that this seemed largely a LASL job, but also noting:

> **I assume we are talking about declassification here and not about opening only for U.S.S.R. inspection; I would like again to call attention to the extreme damage which could be done to our alliances by giving information to the Soviets which is not made available to our allies . . . . I am only pointing out that declassification conflicts with the mistaken idea that nth power nuclear weapons capabilities are inhibited largely by keeping weapons notions secret.**

Bradbury (LASL) noted that providing these designs was considerably more effort than that which was involved with using the Mark VII but:

> **We think this proposed program is much more sensible if the devices are open for inspection and if alpha is measured. If this program is decided upon, we would propose to design the devices and make any necessary assembly tests here, work with Sandia on the packaging, arming, and firing, and be responsible for the determination of alpha.**

Sandia noted that the costs would be somewhat higher and added that all of the warheads previously agreed upon had been received at [redacted] and were currently being modified. All nuclear Black Box and fusing system hardware had been ordered.

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On June 15, Tsarapkin rejected the Black Box concept and insisted upon a system in which device details were revealed. However, the U.S. system continued to coast for a while. On June 16, Starbird, at the request of the U.S. delegation at Geneva, requested that the Laboratories provide step-by-step procedures, assuming the Black Box concept, including the operations and inspections that would be permitted to the Russians. Part of Bradbury's reply was:

> **Comments on the remainder of the proposals are better obtained from those who will have to carry them out. As an example, it is not imagined that radiochemistry is likely to be done at LASL under Soviet surveillance, a situation for which we find ourselves duly grateful. Nor would we care to comment on the embarrassment to the U.S. which might ensue consequent to the presentation or later withdrawal of this proposal. Surely there must now be many experts in Washington on how the U.S. can be embarrassed.**

Livermore continued to press for the polyethylene block method of measuring yield and on June 28, recommended that the present Sandia-designed Whirlaway package under construction be redesigned or that the Livermore Black Box design, which was a variation of their Plowshare design, be used.

At the June 20 Commission meeting, Starbird continued to argue for the Black Box concept and suggested that the AEC request that this concept be pursued further at Geneva. The Commission, however, leaned more toward picking a device whose inner workings could be revealed to the Soviets. The Chairman (McCone) stated that the U.S. would not transmit blueprints of the devices to the Soviets, but would only allow visual inspection. Furthermore, the U.S. would make it clear that cooperation in this endeavor would cease immediately if information on the devices was transmitted to other powers.

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In response to the AEC proposal to the JCAE that we reveal the internal details of the devices to the U.K. and U.S.S.R. only (which would require changing the atomic energy law), the JCAE advised on June 24 and 27 that such a proposal would not pass Congress unless it contained a definite reciprocal feature. In late June and early July the U.S. decided on a tentative position of a three nation nuclear device pool, with the devices to be open for inspection by the participating nations. The U.S. intended to state at Geneva that Soviet rejection of this position would result in strong pressure to proceed unilaterally in its own research program using the Black Box safeguard concept. The British objected to the ultimatum feature and it was deleted.

On June 23, a meeting of technical representatives of the Laboratories concluded that the integral polyethylene method was the best yield measurement, but that modification of the existent Whirlaway Black Boxes would take too long. However, on the same date, Starbird commented that we should stick to one version of the Black Box, and that the target date for stockpiling had now been moved to August 15 (ARPA had recommended a delay to November 1). On the 24th, Starbird reiterated his desire to continue the Whirlaway Black Box program while the question of using a declassified Mark XI was investigated. LASL commented that they could build a Mark XI for any of the yields desired in the program. On July 11, Starbird authorized $536,000 for Project Whirlaway Black Box fabrication and on July 12, the United States made its proposal at Geneva that all three nations contribute old devices to such a stockpile with the device details to be revealed. By now, the first shot of the program had been delayed to October 1. In July, Livermore was fabricating its own Black Box for a Mark VII, incorporating a polyethylene yield measurement, although the Mark VII in the Sandia Whirlaway box was also still a possibility, as was the Mark XI. By August 1, Starbird had decided not to ship the Whirlaway boxes to the agreed upon storage point, Army Ordnance Depot, Wingate, New Mexico, but rather to store them at [redacted] From here on, the question of Black Box use dribbles out into history with the concept still being applied to Lollipop for some time.

Other Aspects Of Vela Uniform

In spite of the Black Box problem, Vela Uniform, commonly called the seismic improvement program in the AEC, grew rapidly during 1960. As has been mentioned, ARPA was assigned the responsibiity for Vela at the beginning of September 1959 and in conjunction with the AFTAC, the Atomic Energy Commission, Livermore, Rand, and others had moved rapidly toward solidifying a program that would satisfy the recommendations of the Berkner Panel. A great deal of work had been done in the second half of 1959 by these agencies to solidify that program. By the beginning of 1960, Project Concerto, the tamped nuclear shots in Nevada, was fairly well defined. In addition, Lollipop, the nuclear shot in Nevada granite, was defined, and Project Ripple, nuclear shots off the Test Site, was fairly clear conceptually. Construction work had been going on for some time on Lollipop and on Cottontail, which was the high-explosive 5-kt shot to be fired in tuff at NTS, and the process of site selection had started for the off-site shots.

The question now seemed to be one of how much enthusiasm the United States was really going to put into this program, how hard it would be pressed, what kind of funding would be attached to it, etc. At the January 11, 1960, Commission meeting, Mr. Graham commented on the importance of the development of a weapon test detection system and stated that in order to test the accuracy of the system, it would be necessary to conduct some underground tests. The Commission, at that meeting, noted that authorization to acquire property and mineral rights for Tatum Salt Dome would

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be covered by the FY 1961 weapons contingency project. On February 24, 1960, as previously noted, the President's Scientific Advisor, Kistiakowsky, informed the Chairman of the Commission of the President's conclusion "that he considers a vigorous and continuing research and development program on the detection of underground and high-altitude explosions to be a matter of high priority" and that he hoped the AEC would find it possible to finance the program for FY 1961 with their existing budget. The February 11, 1960, proposal at Geneva that we agree to a treaty that would ban shots in the atmosphere, but allow them underground while investigating the seismic detection question, obviously put more pressure on the system to prepare for such shots.

During the early part of the year, Livermore planned for Operation Hobo, a set of high-explosive decoupling studies in Tunnel U-12e, and in April they conducted four shots, three of them being 500 pounds of high explosives at depths of 100 feet, 200 feet, and 1,000 feet and the fourth being 1,500 pounds of high explosive at 1,000 feet. These shots were to establish a basis for comparison of decoupled signals in the Nevada tuff with the signals from later shots to be fired in salt.

During the first few months of 1960, the DOD started planning the Groundhog series of high-explosive shots off the NTS.

In spite of overall AEC budget difficulties, Starbird took the bull by the horns in mid-May by transferring most of the remaining test readiness construction money for FY 1960 to the seismic detection program and reassigning most of the underground weapon test sites being prepared in Nevada to the Vela Uniform program.

At the May 1960 General Advisory Committee meeting, Spofford English listed the projected cost of the seismic improvement program of high-explosive and nuclear shots as $2,310,000 in FY 1960 and $13,490,000 in FY 1961.

By mid-May, agreement on the division of responsibility between AEC and ARPA had been reached. The AEC agreed to be responsible for all nuclear shots and the 5-kt high-explosive shot in Nevada, but a large amount of the instrumentation and the interpretation of the diagnostic information would be in the hands of ARPA. Jim Reeves of ALOO was designated manager for the AEC portion of the program. ARPA retained its Advisory Committee (the ad hoc group for detection of nuclear detonations) chaired by Richard Latter, and having as members Frank Press, Kenneth Watson, Allan Donovan, Jack Oliver, Walter Munk, Hugo Bernioff, John Tukey, Roland Herbst, and Conrad Longmire. The program at that time was essentially the previously mentioned Concerto program plus Lollipop. The first shot would be the 5-kt shot in granite in Area 15, NTS (Lollipop). The second shot would be the 5-kt nuclear shot in tuff in Tunnel U-12b (Orchid) and the third shot would be the 5-kt high-explosive detonation in tuff, also in Tunnel U-12b (Cottontail, later called Linen). Others would be a 1/4-kt test (Coffee Pot), a 1.2-kt test (Crystal), and a 40 to 50-kt test (Stingray), all to be in U-12e. The seismic decoupling shots would be done at the Hockley mine in Hockley, Texas.

Work now moved forward to define the program and responsibilities in greater detail. LASL had no desire to be connected with the program at all, but did agree to furnish devices and appropriate diagnostics as mentioned elsewhere. Livermore concluded that they did not want to be responsible for any shots on NTS except Lollipop (possibly partly because the devices, in general, were LASL devices). Thus, the responsibility for the on-site shots other than Lollipop (called as a class Concerto) was given to Sandia. The DOD responsibility was placed in DASA with WET being responsible for the field effort.

By early June 1960, the field organization was beginning to shape up and Lollipop was predicted to be ready for firing by October 15. However, in late June, ARPA recommended a delay until November 1 for Lollipop in order to permit the installation of the maximum number of instrumentation stations. Jim Reeves began dealing directly with the ARPA director, Brig. General A. W. Betts, since ARPA's approval was required for specific site locations. On July 5, Reeves suggested specific sites for five of the shots as e.03a, e.03b, e.08, b.09, and b.07. Tunneling had already started in tunnel b.07, but the other locations had to be considered tentative as there were as yet no firm criteria.

At the July 1, 1960, meeting of the Commission, the Chairman noted that Secretary of Defense Gates favored August 15 for the first shot in a unilateral seismic improvement program. Mr. Kistiakowsky, then Chairman of the Scientific Advisory Committee, had expressed the opinion that this date would be too early and would result in strenuous objections by U.S. seismologists. Howard Brown (AEC Staff) said it would not be feasible to proceed with a meaningful seismic improvement program until the necessary data-gathering instruments could be in place, which would not be before September 15 to October 1.

At the midyear review on July 15, 1960, Harold Brown (Livermore) stated:

> Livermore plans to take no primary responsibility except for the Lollipop shot in granite which may take place later in September 1960, and the decoupling shot which can be carried out sometime in 1961 depending on the size of the salt cavities which may be eventually decided upon. For these experiments LRL plans to take responsibility for staging shots and for coordinating close-in measurements only, immediate and distant measurements being the responsibility of others. For other shots, LRL will serve only in an advisory capacity, concerning ourselves primarily with the theory of coupling of energy from the explosion into the seismic waves. ARPA has overall supervision of the program. If no adequate program of nuclear explosions for measurement of decoupling is authorized, it will be desirable to carry out further chemical explosions at Winfield with gaseous explosives.

By late July 1960, the Lollipop proposed shot date had been changed to October 1, with field exercises to rehearse for the Livermore device configuration about August 10 and for the Whirlaway devices configuration about August 20. No decision had yet been made on which device was to be used, [redacted]

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Thus, on August 9, 1960, Jim Reeves issued a comprehensive planning directive for the AEC portion of the Vela Uniform program. He identified Concerto as those shots, both nuclear and nonnuclear, to be done at NTS, and Ripple as nuclear shots at sites other than the Nevada Test Site. DASA would conduct a separate series of high-explosive shots at locations other than the NTS. The AEC responsibilities were:

a. Fund and accomplish all nuclear shots regardless of location and all high-explosive shots located at the NTS.

b. Obtain close-in measurements necessary to establish cavity behavior and to study the transmission of signals to the immediately surrounding earth on decoupled shots.

c. Make all yield measurements and conduct any postshot drilling required to obtain radioactive samples for this purpose.

d. Designate an AEC project manager with the appropriate responsibilities, which include providing the explosives, site studies, safety, positioning, arming and firing, construction support, etc.

Among other things, DASA was to prepare the consolidated DOD plans, supervise DOD activities at the shot sites, arrange for DOD support, implement DOD directed on-site measurements and designate a DOD associate project manager who would coordinate DOD programs with the AEC, establish DOD readiness for off-site preparations, etc. Jim Reeves was designated project manager and Colonel Leo A. Kiley as associate project manager. Other appointments for Concerto were Bill Allaire as deputy project manager for AEC matters, E. L. Gomel (Sandia Corporation) as coordinator of operations; and John Williamson of REECo as coordinator of base support. The Lawrence Radiation Laboratory was to appoint a technical group leader for Lollipop, whereas Sandia would appoint a technical group director for AEC measurements on the rest of the Concerto shots. DASA would appoint a technical group director for the DOD-directed experiments of Concerto at NTS except Lollipop. A tentative organization for Project Ripple was also given and was very similar to the Concerto organization, except that Lawrence Radiation Laboratory was to appoint the technical director for the programs for which AEC was responsible. The coordinators for Concerto were to be stationed at NTS, whereas the coordinators for Ripple would be headquartered at Sandia Base, New Mexico, and should report for duty no later than September 9, 1960.

Reeves established a site evaluation panel with membership from the AEC, the DOD, the technical contractors (LASL is notable by its absence), and the supporting contractors (EG&G is notable by its absence). He noted three sites as being presently identified as compatible with the Ripple shot schedule, namely the Hockley Mine in Hockley, Texas, about 31 miles northwest of Houston, Texas; the Bruinsburg Salt Dome, Mississippi, midway between Vicksburg and Natchez; and the Tatum Salt Dome, Mississippi, 33 miles southwest of Hattiesburg, Mississippi. Contractor responsibilities were further outlined, with EG&G being responsible for timing and firing of all explosives, REECo being responsible for on-site radiological safety, etc. H&N was to prepare engineering and construction plans and cost estimates and, tentatively, REECo would be responsible for logistical site support and CPFF construction not feasible for lump-sum award. The necessity for further operating agreements, for example, on financial management in fiscal controls, was noted.

Project Concerto ready dates were given as follows: Lollipop, October 1, 1960; Orchid, May 1961; Cottontail, July 1961; Stingray, August 1961; Crystal, September 1961; Porpoise, open; and Coffee Pot, May 1962. Project Ripple was a series of seven nuclear shots to be fired at an unknown depth in salt as shown in Table VII.

In the third week of August, Fred Reines, the discoverer of the neutrino, and Bill Ogle designed a Vela test detection system based on the observation of neutrinos from a nuclear detonation. Neutrinos, having a very small cross section for interaction with other material, pass easily through the entire earth, but unfortunately, because of that small cross section, they are only detected by very large and expensive detectors. It was estimated that a national system consisting of one detector, probably in the Grand Canyon, which would observe a 1-kt detonation anywhere on earth, would cost approximately a trillion dollars, not including the electronics.

TABLE VII
RIPPLE
August 9, 1960

Event	Yield (kt)	Readiness Date	Remarks
Harvest	Withheld Under 5 U.S.C. 552(b)(1) DOE Ex. 1	10/61	Withheld Under 5 U.S.C. 552(b)(1) DOE, Exemption 1
Hayride		10/61	
Hermit		10/61	
Gaucho		12/62	
Geisha		12/62	
Gypsy		12/62	
To Be Announced		07/72	

On August 25, the Lollipop readiness date was slipped from October 1 to November 15. On September 15, Chief DASA defined Groundhog and the assignment of responsibilities for the rest of the community. Groundhog would be conducted entirely by the Department of Defense and would include five 30-ton H.E. shots at a depth of 100 feet and five 100-ton H.E. shots at a depth of 200 feet, with readiness dates ranging from February 1962 to July 1963.

In September of 1960, H. M. Fulley of the Institute for Defense Analyses published a report suggesting that motion of the bomb plasma in the underground cavity produced by a nuclear detonation might generate changes in the electromagnetic field at remote points and, hence, might provide another possible method of detection of underground explosions. This suggestion was not received with favor.

On October 5, Don Shuster of Sandia informed Reeves of the proposed schedule for Cottontail, the 5-kt H.E. detonation. Deliveries would be completed by May of 1961, loading the cavity would start in April of 1961, and a detonation date of June 1, 1961, was planned.

The subject of Lollipop arose early in October and was discussed by Harold Brown and Spofford English at the October 28 Commission meeting. Consideration of containment had raised the question of whether or not the hard granite, contrary to what was expected in tuff or alluvium, would crack, allowing the escape of radioactive gases to the atmosphere. Brown pointed out that the current plan was to fire about December 15 (they must have delayed final installation in order to have this meeting), but various experts in the program were recommending that the shot be fired at a depth of 1,500 feet instead of 950 feet. It was intended that the press and foreign visitors be present at the firing site. Starbird noted that the presence of a plume which might contain radioactivity could result in adverse publicity for the AEC, suggesting, perhaps, that the AEC was unable to confine the radioactivity. Starbird outlined a proposed new schedule that would do Orchid about December 15 and delay Lollipop until a new site could be built. A new shaft for Lollipop would, of course, leave the old shaft available for a future experiment. Starbird noted that if some shot wasn't fired by early January, the seismologists might want to delay the entire schedule until the following spring because of winter background seismic noises. The Commission approved the proposed change in schedule, emphasizing that the first shot should not be delayed beyond January unless absolutely necessary.

On October 4 and 5 of 1960, ARPA sponsored a technical symposium on Vela at the Pentagon in Washington. The attendees, some 300 in number, included representatives from the Department of Defense, other government agencies, industrial companies, universities, and scientific organizations. Among others, talks were given on Vela Uniform in general by Gen. Betts, detection by Carl Romney, worldwide standard seismic network by L. M. Murphey, the explosion series by Theodore George (who did not mention Groundhog), source measurements by Chuck Violet of Livermore, decoupling by Al Latter, and results of Project Cowboy by Glen Werth.

By late October, some of the names had changed. Concerto had become Project Shade and Ripple had become Dribble and was now definitely planned for Tatum Dome, Mississippi. The previously unspecified shot to be fired in an active seismic area had now been given the name Shoal. Dribble had been changed to six nuclear shots (vice the previous eight) to be fired at a depth of 2,500 feet. LASL was trying to dream up an acceptable direct sampling technique for a 5-kt nuclear explosion underground.

By fall 1960 the AEC had received an FY 1961 supplemental appropriation of $32,500,000 for Vela. The Department of Defense had committed $7,460,000 in FY 1960 and had programmed $33,340,000 to be spent in FY 1961. A great portion of the DOD money was to be spent through universities and private contractors.

By October 28, 1960, the AEC, DMA, and DASA had authorized final construction for the Orchid event (5 kt in tuff, tamped) to meet a readiness date of December 15, 1960, a very tight schedule.

Some comments on the procedures for firing an underground shot in a tunnel will, perhaps, aid in understanding the continued delays. After "field construction," the phase during which the construction contractor prepared the site for laboratory occupation, there was a period of a month or two months or even longer, during which the equipment for whatever experiments were to be performed was installed and tested. In certain circumstances, the equipment installation phase could overlap the field construction period. After the technical installation was finished and checked out, the device itself could be brought in, last-minute checks run, and then stemming could proceed. The process of stemming could take from a week to as much as a month or so. Because digging back to recover the device was a large and obnoxious job, very few people in the system wanted to bury it without having received firing authorization. A very few times during a later period of underground testing, devices were emplaced before authority to fire was received, and the results were, on occasion, embarrassing. Thus, while the field organization could bring a site up to something like six-week or one-month readiness, it, in general, could not go past that point without receiving authority to fire. Such authority, in this period of time, was continually delayed and, of course, always delayed as late as possible. Thus, we see, time after time, shot preparation progressing to within a month or two of the planned firing date before delays began because permission to fire had not been received. In the Vela Uniform program during this period, there was, of course, further confusion on which device was to be fired, [redacted] or something else. Since device selection affected the zero room geometry, the firing circuitry, etc., additional delays were introduced. Lastly, since the field organizations were there to carry out experiments, any delay might be seen as an opportunity to change or improve some part of the experimental setup. These changes could lead to the expenditure of further effort and time.

Withheld Under 5 U.S.C. 552 (b)(3) DOE Ex. 3

During the summer and fall of 1960, several high-explosive detonations for the Plowshare program were fired in Nevada. While not strictly part of the Vela Uniform program, some of these did add to the data base for Vela Uniform. Scooter, a half kiloton of high explosive buried 125 feet deep, was fired in October of 1960.

In late October, the location of Orchid was changed from U-12b.09 to U-12e.04. Other changes were made during this period as a result of continued effort by ALOO and LRL to site the proposed detonations so as to get the most out of the overall underground complex. Some of the initial site choices had been such that firing the planned shots would limit the use of already constructed zero points.

Several things in late 1960 reduced the likelihood of firing Vela Uniform shots in the near future. The Eisenhower administration was reluctant to act after the election of John F. Kennedy as President of the U.S. in November of 1960. The problem of which device to use, Whirlaway [redacted] had not been settled. The Geneva Conference was to recess on December 5, so there was little time to achieve U.S.S.R. agreement to Vela Uniform shots, and Tsarapkin had made it clear that he objected to the decoupling shot program.

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Thus, on November 16, Starbird advised that the first shot of Project Shade would now be Orchid, to be fired on April 15, 1961, with Lollipop following on about June 15 at the new depth of 1,500 feet. The Lollipop hole would be drilled to 36-inch i.d. to accommodate the Sandia Whirlaway box.

A November 20 memo from DASA to DDR&E defined the Groundhog series in a little more detail. It was listed as a series of 30-ton and 100-ton high-explosive detonations at depths of 100 and 200 feet to be conducted in the summer of 1961 at an unspecified site.

A number of chemical explosions of up to 100 tons are required to investigate relative coupling in various media, to investigate the effect of increasing depth of burial in one or more media, to provide additional comparisons of chemical versus nuclear explosions, and to provide a direct comparison with small earthquakes in a seismic region.

Table VIII details Shade and Dribble.

TABLE VIII

SHADE AND DRIBBLE
November 20, 1960

Name	Yield (kt)	Medium	Depth (ft)	Site	Firing Date
Lollipop	5.0	Granite	1900	NTS	06/15/61
Orchid	5.0	Tuff	900	NTS	04/15/61
Linen	5.0 (HE)	Tuff	900	NTS	09/15/61
Stingray	25-50	Tuff	2000	NTS	11/01/61
Crystal	1.0	Tuff	900	NTS	08/01/61
Record[a]	0.1	Salt	Decoupled		10/61
Hayride[a]	0.5	Salt	Overdriven		10/61
Hermit[a]	0.1	Salt	Tamped		10/61
Muslin	0.25	Tuff	900	NTS	05/62
Shoal[a]	5.0			Calif.	07/62
Gaucho[a]	5.0	Salt	Decoupled		12/62
Greenbean[a]	25.0	Salt	Overdriven		12/62
Tipsy[a]	5.0	Salt	Tamped		12/62
Porpoise	5.0	Tuff	5-10,000	NTS	

[a]Dribble

At the December 2, 1960, Commission meeting, it was noted that the Bureau of the Budget had suggested a reduction of $10,000,000 for the Vela Program in FY 1962. "The AEC staff deemed it appropriate to appeal this recommended reduction. General Starbird said if such a reduction was permitted, the deep shots and the decoupling shots recommended by technicians and (the) seismic improvement program would be delayed considerably." The Commission agreed to appeal the $10,000,000 reduction.

On the last day of the year the status of tunnel construction for Shade was as shown in Table IX. [redacted]

5 U.S.C. 552(b)(3)
EXEMPTION 3, D.O.E.

TABLE IX
SHADE STATUS
December 31, 1960

Name	Yield (kt)	Status
Orchid	5.0	Construction complete
Crystal	1.0	Construction complete
Lollipop	5.0	60% of exploratory holes complete
Linen (previously Cottontail)	5.0 (HE)	Extended shaft and sphere, 67% complete
Stingray	25-50	No site selection.
Porpoise	5.0 (deep)	No site selection.
Muslin	0.25	Tunnel advancement complete for U-12e.03 but it is noted that the event may be canceled.

NTS Readiness Effort Shifts to Seismic Detection, Mid-1960

In early May 1960, the Nevada program was the only viable readiness field program available to the AEC. The LRL had a quick readiness program of four shots for which the construction had been approved and was going forward, and the LASL had been given authority to deepen some holes and drill others to provide for more zero points. However, at this time, the readiness effort began to degrade rapidly. As mentioned before, on February 24, the President's Science Advisor had informed the Chairman of the Commission that the President considered a vigorous and continuing research and development program on the detection of underground and high-altitude explosions to be a matter of high priority. During the intervening time, the U.S. had informed Russia of its proposed Vela Uniform program and was about to join the agreed-upon discussions of the Seismic Research Program Advisory Group to be held on May 11, 1960. Eisenhower had publicly announced the Vela underground program on May 7. On May 9 Colonel O'Brien of DMA and Allaire of ALOO agreed that Reynolds Electric and H&N should start "shifting gears" from the present program to the detection program. In addition, they agreed to cancel the bid on the construction of LASL holes and the other Area 3 items directly associated with those holes. They further agreed that ALOO should check with LASL to see if the alpha trailers were still desired for other possible uses. On May 13, ALOO sent to O'Brien the estimated savings based on immediately stopping all weapons program construction. The total savings for FY 1960 could be $1,625,000. On May 18, a Starbird message to the field stated:

> It appears now that the Succotash activities and expenditures for NTS construction and procurement should be stopped immediately except for completion of digging for the U-12e.07 drift and vertical shaft. (If there is any other Succotash effort that may contribute directly to the seismic program it should be made known to me.) I would plan, therefore, to direct shortly that the necessary action be taken to terminate such activity and that the funds concerned be diverted to cover the initial funds of the seismic improvement program. I request that ALOO advise me at the earliest practical time as to the savings that can be accomplished in FY 1960, thereby, and the amount required for FY 1960 seismic improvement activities.

Succotash was the Livermore weapons test readiness effort. The LASL effort, as noted, had already been stopped. The field reacted quickly. On May 19, Harold Brown

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of Livermore told Starbird:

> The proposed program, including the 50 kt in e.07 and the 1.2 kt in e.03, wipes out three of four sites of the readiness program. In addition, the HE shot will very probably make the B tunnel unavailable for a period of six months to a year . . . .In case the international situation were to lead at some future time to a decision to resume underground testing, there would then be a delay of 3 to 6 months beyond the 40- to 50-day readiness which we had previously planned. We consider this very undesirable . . . . Two possible sites in the E tunnel for the 50-kt and the 1.2-kt shots should be obtained by additional tunneling funded from the seismic improvement program, as would be the case for experiments and locations other than at NTS . . . . We will, in the near future, make up a proposal for additional work to put NTS into the same state of readiness capability which we have previously proposed; the details of such a proposal will obviously have to await firm and final arrangements for the seismic program.

It is interesting to note that in the Commission meeting of May 18, which Starbird attended, the subject of this shift did not come up. However, Mr. Graham did say that the Commission was committed to proceed with the seismic improvement program and it was remarked that in view of the failure of the Paris Summit Conference, Congress might not approve funds for the test detection program.

At this point in time (mid-May 1960), the site for [redacted] shot (U-12b.09) was just short of construction completion and a month short of scientific construction completion. The site for the [redacted] was about two months short of construction completion and the site for Chiffon [redacted] U-12e.03) was roughly two months from completion.

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From this point on, the effort devoted to test readiness began to drop off appreciably. However, some work continued. Livermore and EG&G completed on May 23 their Project Tattoo, which was the field environmental test of a new underground prompt diagnostic telemetry system in which the data was partly reduced inside the tunnel and then transmitted over hardwire to remote instrumentation stations. The test used simulated gamma and neutron signals similar to those that might be received from a nuclear device.

During July and August 1960, the USGS groundwater investigation of Yucca and Frenchman Flats continued with the drilling of new water wells. Some seven wells were being drilled from 900 to 1,850 feet deep, the six deepest wells being in Yucca Flat and a 900-foot well in Frenchman Flats. The rate of drilling was such as to produce two new wells per month.

In spite of the shift to Vela, Livermore continued work on test readiness. After a tour of NTS on July 12 with members of LRL and ALOO, Starbird requested that Livermore send in a new plan for Succotash (test readiness) based on the assumption that the Vela Uniform program would use the existing tunnels. He asked that the plan be sent in as early as practical, but stressed that this did not imply a strong possibility of getting money for added weapon test readiness. Harold Brown replied in mid-August with a detailed plan to get into a state of 30 to 60 day readiness the same four shots as previously decided upon. Costs would be approximately $900,000 on the basis of a 40-hour workweek. Internal planning on the physics design for the measurements to be made was essentially complete at this time.

In mid-November 1960, Reeves (ALOO) expressed his concern to Starbird on the growing U.S. inability to resume weapons testing in a short time. He requested guidance from DMA in order that he could prepare a plan for future weapons test capability. The development of such a plan following DMA guidelines would then permit DMA to authorize such portions of the plan as might be feasible considering budget restrictions and political implications. There was no immediate response.

Thus, the field expenditures for weapons test readiness, and most of what few sites were available were switched to Vela Uniform in mid-1960.

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Device Predictions, Mid-1960

At the end of April, Starbird asked the Laboratories for their opinions on what could be accomplished for the next few years in weapon development if certain kinds of operations were allowed, such as decoupled shots up to 50 kt, outer space shots, etc. The question was apparently triggered by a recent high-level briefing by Teller on the advantages of testing.

Harold Brown speculated for Livermore by discussing the possible yields that might be developed as a function of weight up to a 6000 pound, 50-megaton device, and the possible gains that could come about by testing at low yields underground or at high yields in deep space. He noted the many gains that might be achieved clandestinely by the U.S.S.R., and that in particular:

> The tactical weapons which we consider would be capable of producing an equal or greater imbalance in nuclear capability can all be done with tests in the kiloton range of whose easy concealability there would be no question.

In general, Brown and Teller were optimistic as to the advances that could be made with almost any kind of testing.

Bradbury also replied, stating that in his opinion, things were less optimistic than Teller seemed to feel and:

> In short, nothing has occurred in the last year and a half to change my own opinion regarding the extent of weapon gains possible with limited testing or even with unlimited testing. I am much less optimistic than Teller on both points . . .

In mid-May Mark and Bethe continued an exchange on the subject. The circumstance appeared to be roughly as appears in Table X below. "Essential weight" is the nuclear device component weight but not including firing set, bottle, etc. Case I was presumably numbers used by Teller in April as what might (optimistically? realistically?) be expected in a few years if testing were resumed. Presumably these advances could be obtained by testing in the few kt range for all but the largest yields, and they might be obtained without having to test above 100 kt. Case II was a November 1959 LRL prediction for 1970 if testing were allowed, and Case III was predicted at the same time for 1965. Case IV was intended to represent the "state of the art" at the time but in a slightly advanced form. The author has been unable to resist adding the "modern" case, representing approximately current thought and experience in 1980.

It was clear that some of the differences could come about by different assumptions. For example, the low weight row is controlled largely by one point safety considerations. But the large-weight, large-yield predictions of Case I pretty clearly required a "break through" which has apparently not yet come about. Mark concluded that surely some advancement would come over the "state of the art" (Case IV), and by detailed argument ended up concluding that Case III represented roughly "the present frontier between optimistic science and science fiction", and that the region of Case III "represents the area towards which and possibly into which one could work". Mark remarked that even Foster did not seem to believe the most optimistic case.

Bradbury later remarked to Starbird that he felt quite gloomy as to the future based on only limited-yield underground testing and felt that even "state-of-the-art" (Case IV) was about as adventuresome as the LASL was willing to imagine at that point.

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5 U.S.C. 552 (b) (3)
DOE, Exemption 3

At the end of June, Starbird asked Bradbury and Brown for their judgement of the relative disadvantage of the U.S. vis-a-vis the U.S.S.R. in weapons development progress assuming that testing were to resume with the Russians testing in any manner they wished, but with the U.S. testing only underground or both underground and in space above 100,000 kilometers. Livermore replied that in their opinion, if we tested only underground, we would have relatively little disadvantage and that disadvantage would be a function of the yield range, with us being at greater disadvantage for high yields where we couldn't test underground so easily (if at all) in other than a scaled design configuration. If we could test both underground and at very high altitudes there would be a negligible disadvantage. The LASL, however, was more pessimistic. Bradbury felt that the first effect would be a time lag between ourselves and the Soviets of perhaps six months for underground to perhaps two years for high-altitude testing, while we developed the techniques. He also felt that even after that, we would fall behind at some rate unless we poured tremendous amounts of money and effort into our program to stay up with the Russian development accomplishments.

In early July, Starbird asked Brown and Bradbury to comment to the AEC General Advisory Committee (which would meet in Washington on July 26-28) on the U.S.S.R. capability in weapons development during the moratorium. Bradbury immediately replied by TWX that he hadn't the faintest idea. However, he would guess that since at the beginning of the moratorium we were apparently ahead of the Russians, the rate of progress should be a little less than ours, partly because they did not seem to diagnose their shots as carefully as we. If that estimate was wrong, then he would guess that they had the same capability for making progress during the last two years as we had. As for cheating, he stated that he had no idea whatsoever if they had been cheating nor any technical basis on which to hazard a guess. He sent Mark to the meeting, at which John Foster presented the Livermore feelings on the subject. Foster stated that in his opinion, the U.S. had no assurance that the Soviets were not now accomplishing improvements by actual testing. He then discussed the various kinds of improvements that could be made by testing, making the point that from now on, the yield versus weight question would probably be less important as compared with other aspects of the warhead, such as vulnerability, effects of the shot, etc.

Withheld Under
5 U.S.C. 552 (b) (3)
DOE, Exemption 3

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Withheld Under
5 U.S.C. 552 (b) (3)
5 U.S.C. 552 (b) (1)
DOE, EXEMPTIONS 1 & 3

[redacted] Bethe commented that "such development is probably impossible in any militarily useful form." He had no comment on the Russian capabilities or possibilities in this class of device.

Vela Hotel, July-December 1960

By mid-July of 1960 Livermore had been funded by ARPA to look at some of the backgrounds that might affect Vela Hotel measurements in space. They were also seeking additional funding to expand their program to measure the neutron albedo of the earth's atmosphere, and x-ray, gamma ray and particle backgrounds at altitudes characteristic of both high and low orbit satellites. At the same time LASL and Sandia were building small "piggy back" packages scheduled for delivery in August in order to be launched on the JPL Ranger A-1 probe in April 1961. General Betts, ARPA Director, had arranged for joint AFSWC-LASL payloads to be flown on a dedicated inexpensive version of the TS609 A Scout system known as the 2356 configuration. While there was not as yet overall approval or funding for the AEC Vela Hotel effort, Starbird did arrange in late July to fund LASL and Sandia at a level of $1,000,000 each for FY 1961.

In late August LASL and Sandia summarized their interim capability status in a document entitled "ICOS and BLICOS". The document listed the rocket probe and piggyback flights planned, as well as the high altitude balloon flights to determine the effects of cosmic rays on the x-ray detectors. The overall schedule included the following milestones: delivery of several piggyback packages to the NASA Ranger program beginning almost immediately, launch of AEC detector and logic packages on Journeyman-B rockets beginning in October of 1960, and balloon flights in February and March of 1961. The balloon flights would provide data on parameters for the ICOS alarm system. The Journeyman-B flights would carry x-ray and gamma-ray scintillation detectors to about 30,000 miles altitude to look at the short duration pulse background.

At the previously mentioned symposium on Vela in early October 1960 the ARPA Vela Hotel Project Manager, Major John A. Poulson, described the Vela Hotel system currently being considered for full development as a combination of two major systems. The first, known as the "Far Earth" system and intended to detect unshielded detonations taking place in the region from about 30 to about 200 million kilometers altitude, involved six satellites, three each installed in two orbital planes at right angles to one another. The satellite orbit perigees would be outside the Van Allen belts. Each satellite would carry detectors for neutrons, prompt and delayed gamma rays, and x-rays. The other major system, known as the "Earth Proximity Solar Satellite System", would include a number of solar orbiting satellites instrumented only for prompt gamma and x-ray detection. It was thought that a shielded detonation could be detected to about ten million miles by observation of the prompt gammas, so that by putting enough satellites in solar orbit a treaty violator would have to test as far as 70 million miles from earth with a shielded detonation, which would be quite an effort. The program would be conducted in three phases. The first step would use existing components and technology to develop an early detection capability; then more advanced technology would be used to counteract the possible shielding an evader could employ, and the last step would be to provide a capability to collect some diagnostic data from a detected explosion. The overall program budget would be about 100 million dollars to be spent over a four year period.

On October 18, Starbird informed Bradbury of the current status of Vela Hotel funding. Having told Starbird on August 10 that a decision on proceeding with Vela Hotel would come within several weeks, ARPA had just advised Starbird that the program decision had still not been made, but ARPA was continuing to press for it. Starbird reaffirmed to Bradbury that LASL should continue the previous course of attempting to fly piggyback experiments on NASA and DOD missile flights, probes, and satellites. Starbird also stated that AEC Chairman McCone, in a letter to Secretary of Defense Gates on October 12, had requested DOD assistance in lifting AEC instrumentation packages into space.

On November 14, AFSWC published a report on their Blue Scout project entitled "Operation No. 10-61, Project Blue Scout, Jr." In an effort to develop a lower-cost sounding rocket, specifically to support preliminary developmental testing of instrumentation for detection satellites, AFSWC had developed the solid propellant vehicle, Blue Scout, Jr. (TS 609A). The initial launches, which were at least seven months away at this time, were to be performed by the Air Force Ballistic Missile Division (AFBMD). The first launch was to be from the Atlantic Missile Range (AMR) and the next two from the Pacific Missile Range (PMR).

LASL and Sandia Vela Hotel plans for the next two fiscal years were sent to Starbird on November 23, 1960. The overall program included detector packages, to be flown either piggyback or dedicated, aboard DOD small rockets and satellites from late 1960 through late 1962; three balloon flights to 120,000 feet in February and March 1961 (for BLICOS); and, perhaps most significantly, the launch of two 160-pound, modified ICOS, prototype protection satellites to 100,000 kilometers altitude, the first in January 1962. The program was now emphasizing the x-ray detection system which had seemed the most promising to the Panofsky Panel. The report also stated that the growth of the satellite detection system could now take one of two different routes:

> a. The moratorium may be maintained, resulting in the primary emphasis being placed on extending the detection range, or,
>
> b. we may return to testing, causing the emphasis to be placed on intelligence diagnostics for relatively close-in detonations.

The Labs were incorporating both of these possibilities in their thinking for future developments. The above-described program was planned to be completed within the then present DMA authorized budget for the two Laboratories of $1.86 million for FY 1961 and $2 million for FY 1962.

About this time, in the last month or so of 1960, the Department of Defense, who were providing the major part of the funding, reduced its effort in Vela Hotel R&D. This action was directed by the Secretary of Defense, who felt that the general objective should be to obtain as much pertinent experimental data as possible in order to increase basic knowledge and understanding of the physical phenomena affecting detection. Thus, as related by Starbird to ALOO and the Labs on November 25, ARPA, after discussions with LASL and Sandia, issued a new draft order to ARDC, initiating the reduced-scope program. Starbird requested that addressees comment on the new order which changed the concept from a four-year $100,000,000 system development program to one with much-reduced funding aimed at obtaining experimental data at the earliest practicable date. The new funding, exclusive of what NASA may have had, gave DOD and AEC only $13.8 million total for FY 1961 and 1962; $5 million for DOD and $1.9 million for AEC in each fiscal year [redacted] ARPA requested that ARDC, AEC, and NASA jointly prepare an updated development and funding plan by early January. Hertford replied within a few days to Starbird that the two Labs felt the general approach was reasonable and acceptable. He also pointed out that meeting the ARPA request for a new plan by January required that a group to work out the details must be formed immediately. [redacted]

Withheld Under 5 U.S.C. 552(b)(1) DOE, EXEMPTION 1

The joint meeting to work up a detailed, reduced-scope program for Vela Hotel took place at AFBMD in Los Angeles on December 15 and 16. Attendees included, among others, Taschek and Austin McGuire of LASL, Don Shuster and Jim Scott of Sandia, Steve White of Livermore, and Lew Allen of AFSWC. Minutes of the meeting indicated that there was disagreement about the distribution of funds and about responsibility for instrumentation packages and rocket flights. [redacted]

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Vela Sierra, Late 1960

By early July theoretical predictions for the ground-based detection system of high altitude detonations indicated that placing the instruments about 1700 kilometers apart on the earth's surface would produce a system with essentially no blind spots. EG&G was committed to delivery of a first prototype of the air fluorescence system to LASL by August 1, and discussions were taking place concerning the design and test of a "direct" optical system to measure visible radiation from expanding bomb debris. The "Direct Optical" system was under order within a couple of months.

The first air fluorescence system was actually delivered on August 16 and was being used to gather data on discrimination between lightning and nuclear signals by the end of the month. A second prototype of the system was to be built while the first was being tested at LASL, with the intent of then testing both at Fairbanks, Alaska and Thule, Greenland between November 15 and December 15. Further reduction of data from the Teak shot of Hardtack was yielding phenomenological information and understanding of great help in the design of the system. By the end of October enough experimental data from the lightning experiments were available to design the necessary changes in the system to discriminate against lightning, and two new prototypes incorporating these changes were due by December 1.

The first new prototype system was actually shipped to Ladd Air Force Base in Fairbanks in late December, and "Project Big Moon", to test it against auroral background, was in operation in January 1961. The second system was shipped to LASL, first for further test, including observation of solar flares, and then to Thule for test in "Operation Brass Ring."

As Vela program managers, ARPA had responsibility to design an overall detection capability to as high a degree as feasible, with no particular attention to cost. However, AFTAC had responsibility to actually build, install, and operate a detection system quickly, under rather severe budget restrictions. Thus LASL was delivering to AFTAC the criteria for a complete direct optical and fluorescence system that would cost about $200,000 per station, and to other agencies the design of a system limited only by the physics involved.

While the optical techniques mentioned above were being developed, other techniques to carry out the Vela Sierra mission were also being investigated. Since one of the obvious effects of a high altitude detonation is to produce free electrons in the upper atmosphere, various methods of observing the effects of such free electrons were being investigated. In particular, the possibility of possible phase shifts in very-low-frequency radio propagation was being studied at the Navy Electronics Laboratory, geomagnetic perturbations were being studied at the Signal Corps Research and Development laboratories, other radio techniques were being investigated at the Institute for Defense Analysis, and the use of riometers was being pursued at the Stanford Research Institute. [redacted]

Withheld under 5 U.S.C. 552(b)(1) 1.3(a)(5) DOE EXEMPTION 1

The AFTAC program clearly involved the need of stations in foreign nations. In late 1960 AFTAC queried Starbird concerning any problems that might come about if foreign nationals were used to man the stations. Starbird passed the question on to the Laboratories, expressing particular concern as to whether or not, especially in the case of the observation of electromagnetic (EM) signals, such use would require the transmittal of restricted data (RD) to the foreign nationals. Bradbury replied that complex sophisticated systems to observe diagnostic signals might reveal RD, but less elaborate equipment to observe gross signals probably would not. He went on to comment that the use of EM seemed to apply to atmospheric detonations, in which case if the bomb was big enough to be observed by EM at great distances we would probably also pick up debris, and if it were too small to find debris, EM probably wouldn't

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see it either unless the stations were real close, in which case "watching the sky with a Bhangmeter may be a better technique."

Deep Space, Later-Half 1960

Progress on the Advanced System for Weapons Testing (ASWT) was reported to LASL by their representative on system studies, Don Westervelt, on July 5. The assumption was that the initial objective of the system would be [redacted] The first flight of that system was not scheduled until mid-1961 so the system would not be adequately tested until early 1963. The lead time for the AEC part of the test was 18 months. The military had given attention primarily to launch sites in Florida and California, and the AEC, because of nuclear safety hazards, had pressed for additional consideration of an island launch site. Thus, the final report would include discussion of the possible use of Eniwetok, Johnston Island, and Christmas Island, with the Air Force having a strong preference for Eniwetok. [redacted] The final program document on ASWT was forwarded to ARDC headquarters and to DMA on September 7, 1960. Subsequently, General Harrison, Deputy Chief of DASA, sent a letter to General Starbird stating that DASA was recommending to the DOD that the AEC be asked to join with the DOD in seeking executive approval for proceeding with the development of the ASWT capability. On October 5, Starbird advised Hertford that briefings for AEC and DOD on the ASWT program had not led to a commitment on the part of the AEC to attain this capability, and that for FY 1961 and FY 1962, the weapons budget would not reflect funding in support of ASWT beyond capability studies. Starbird directed Hertford to continue conceptual studies related to the engineering and safety aspects of the AEC payload.

Withheld Under 5 U.S.C. 552(b)(3)

Domestic and International Political Developments, May-December 1960

May 1960 was a month of change in the mood and trend of the Geneva test ban talks. Whereas, as discussed above, certain exchanges, especially at high level, through the February to May period had created a feeling that perhaps a signable treaty might be in the offing, events in Geneva and elsewhere in May substantially decreased these hopes. Just how much effect the infamous U-2 incident had on the test ban agreement is debatable, but the timing of that international incident is certainly noteworthy in the light of what happened at Geneva. On May 1, an American U-2 reconnaissance aircraft went down in Russian territory and both the pilot (Francis Gary Powers) and the aircraft fell into Soviet hands. After several exchanges between the U.S. and Soviet governments over the conditions of the incident and the Soviet demands for American compensation of some sort, the several world leaders went to Paris for the long-planned summit meeting on May 16. Khrushchev there confronted Eisenhower with a demand for discontinuance of the U-2 flights and punishment of those responsible. Eisenhower refused to comply with some of the demands and, consequently, Khrushchev refused to participate in the meeting. Thus ended the summit, which was to have addressed some of the hardest issues in the path of a test ban agreement. Immediately afterwards, in response to a question at the

May 18 press conference as to whether the Soviets would continue to participate at Geneva, Khrushchev replied that they would continue the negotiations, feeling that they had recently provided some clarification to the Americans of the Russian stance, and "if they understand that, then there is a possibility to reach agreement on the discontinuance of tests. But if Eisenhower threatens that he will continue testing, then we, too, will follow suit until the whole world learns who are the true guilty parties and who is resisting agreement." Eisenhower stated on May 25 that the U.S. would "not back away on account of recent events, from the efforts or commitments" that it had undertaken in the nuclear test and disarmament negotiations.

This strong shift in Soviet mood manifested at the Paris summit conference was echoed in the Geneva test ban conference during the rest of the year. In Eisenhower's words in his autobiography several years later:

> This completely ridiculous gesture (Tsarapkin's May 27 exposition of the new Soviet position) terminated, so far as I was concerned, the dreary exercise. In our years of effort, there had been accomplishment--unfortunately, too much of it theoretical--but it was obvious that for the moment we had reached a blind alley. . . .It was now clear that further voluntary suspension of testing was useless and would, if continued, place us in a disadvantageous position. Prudence demanded a resumption of testing, and except for the fact that my administration was reaching its end, I would have immediately announced such a decision. However, I felt that if the incoming President had a different judgment, it would be unwise to tie his hands by my action at this late date. Accordingly, we did no more testing during the remaining few months of my administration, but I emphasised to President-elect Kennedy my conviction that our nation should resume needed tests without delay.

Eisenhower also decided that if Nixon won the Presidential election he would announce before the inauguration that the U.S. would resume testing.

The authors of *Diplomats, Scientists, and Politicians* present their own opinion of the post-summit situation in a chapter with the telltale title "The Collapse of the Conference." In part it states:

> The attempt to solve the differences between East and West relating to the technical aspects of a control system for a test ban had failed. Agreement had not been achieved, and the attempt to bridge the disagreement through political compromise and scientific research had collapsed. . . .President Eisenhower--like many Americans--was greatly disheartened by the collapse of the summit meeting and the obvious stalemate in the nuclear test ban talks. He virtually gave up hope of achieving a test ban treaty, and his views were shared by a number of American policy makers. Nevertheless, Western, and more particularly American, policy seemed almost to have achieved a momentum of its own, and the policies established earlier in the negotiations were pursued with very little modification.

Meanwhile, the Seismic Research Program Advisory Group, the experts from the three countries who were to work out a joint or coordinated underground detection research program, began their meeting at Geneva on May 11. The Americans opened by presenting their planned Vela Uniform program which included about a dozen underground nuclear tests and several underground HE explosions. Subsequently there was a presentation of the more modest British research program. Later, over a period of a few days, the Soviets presented a discussion of a somewhat ambiguous research effort that seemed clearly at times to point to a significant program of underground explosions and at times included reference to "a certain number of coordinated nuclear explosions" which, at one time, the Russians seemed to be saying, would be carried out by them. Following questioning, the Soviet's proposed program was clarified to indicate that no nuclear explosions would be planned within the Soviet Union. Discussion of these programs and ideas went on through the 24th of May and past the abrupt end of the summit conference. Then, on May 27, before the Geneva talks had reached a clear conclusion, Soviet Ambassador Tsarapkin announced, at what was the first meeting of the diplomatic conference since May 12, that the Soviets had never doubted the validity of the Conference of Experts' report and had agreed to come to these technical discussions only because of U.S. insistence. He directly contradicted some of the statements made by Soviet members of the Seismic Research Program Advisory Group in stating that the Soviet Union saw "no need for undertaking any research or experiments on its own territory." Furthermore, he demanded that USSR scientists participate fully in any underground tests in Western territory and that there must be guarantees that these explosions would not be used for military purposes. Following this meeting, the technical advisory group met only once more, apparently, and ended up publishing only individual private reports to their own diplomatic delegations.

At the July 26 to 28 meeting of the AEC's General Advisory Committee, Dr. John Foster of Livermore stated his opinion that there was no assurance that the Soviets were not now testing clandestinely. The committee's public record included the conclusion that they were "convinced that it is technically possible for the U.S.S.R. to conduct, without serious risk of detection, significant weapons tests under the current test moratorium." Chairman McCone of the AEC emphasized in the last several months of 1960 that the negotiations could not continue indefinitely in a *de facto* moratorium with a total lack of safeguards. He also decried our lack of knowledge as to whether the Soviets had been testing or not.

The Geneva Conference continued through August 22 and then, following a recess, had another session from September 27 through December 5. In the break between the two sessions, Ambassador James J. Wadsworth, who had served from the beginning as U.S. representative, left to serve as U.S. Representative to the United Nations and was replaced by Charles C. Stelle as acting representative. It can be generally stated that although some of the most important issues were addressed--number of inspections, localization of areas which qualify for inspection, staffing of the control post, details of the detection and identification system and its installation, and appointment of deputy administrators to the control commission--almost no real progress and no important compromises were reached at the Geneva talks through the remainder of 1960.

During this same period other pressures against an indefinitely continued moratorium began to appear. On June 13, 1960, General Nathan F. Twining, Chairman of the Joint Chiefs of Staff, sent a memorandum to James Douglas, then acting Secretary of Defense, to present the JCS feelings on the important issues and status of the Geneva talks. He referred to an August 21, 1959, JCS memorandum to the Secretary of Defense in which the Joint Chiefs presented their views that "an adequate military posture for the U.S. will not be attained until there is available a complete spectrum of weapons compatible with modern delivery systems which will make it possible to apply selectively adequate force against any threat." The Joint Chiefs now recognized that the U.S. would not achieve such a spectrum of weapons if an enforceable test ban agreement were concluded and implemented, but also recognized the theoretical advantage to the U.S. "militarily" if a controllable test ban were reached. Twining emphasized that unless such a test ban treaty could guarantee test cessation in the Sino-Soviet bloc (not just Soviet) and thus result in the desired effect on Soviet weapons and stockpile development, further U.S. testing to develop new weapons would be vital, especially as it would increasingly affect sophistication of the existing and potential weapons systems, including the antiballistic missile. His statement of the present JCS position continued, stating that the JCS:

> . . . believe it essential to the maintenance of our nuclear deterrent to periodically detonate weapons to test systems and techniques to the employment of nuclear weapons to ensure operational realiability, and to further sophisticate weapons systems. . . . The most important matter of concern now, however, is the apparent movement

of the U.S. away from a safeguarded treaty to one of "good faith" which has always been the Soviet approach. A prolonged moratorium without satisfactory development of a reliable control system achieves essentially the same results for the Soviets as an agreed and ratified treaty.

Chairman McCone, on July 21, wrote to Secretary of Defense Thomas Gates, addressing the problem attendant to the proper use of scientific data and scientific theory in the political and technical negotiations toward a treaty. As an example of his concern, McCone cited a recent Rand report entitled "The Capability of a Seismic System," which had been prepared largely on the basis of theory. When Albert Latter briefed the Principals on the study prior to their departure for the Paris summit meeting in May, "no one clearly expressed any such reservation or qualification. Hence, many who had had the briefing accepted the conclusions as authentic and dependable." McCone expressed concern that time and again in the diplomatic negotiations, beginning perhaps with the Conference of Experts, theoretical studies had been accepted as adequate, where the conclusions really should only have been considered dependable after further experimentation. McCone warned against this practice and sent a copy of the letter not only to the Secretary of Defense but to the other Principals and to General Goodpaster on President Eisenhower's White House Staff. Gates replied on August 10, stating his full agreement and his intention to make clear at the Principal's discussion his "feeling that the effectiveness of the proposed system should be proven by experimentation and research before the U.S. considers a treaty commitment to prohibit nuclear weapons tests in that environment."

On September 14, just before Geneva talks reconvened, Chairman McCone sent a letter to Secretary of State Christian Herter stating his strong personal feelings about how far the negotiations had come and how this affected the U.S. posture under the present uncontrolled moratorium. He felt that:

> . . . as a matter of policy, we should pursue in an aggressive manner during the month of October the unresolved questions which in the final analysis determine whether a satisfactory and adequately safeguarded control system will or will not be agreed to by the Soviets. I would hope that we could, by this negotiating tactic, reach a conclusion not too long after the meeting reconvenes as to whether the Soviets intend to conclude a reasonable treaty or, on the contrary, are employing the tactics of a protracted negotiation with no intention of settling the critical issues which separate us. If such is the case, we will face the necessity for an early decision as to whether we should continue the de facto moratorium without safeguards and without any reliable assurances that the other side is adhering to the same rules, or whether we should adopt a new and independent course of action.

Anticipating the forthcoming session of the Geneva talks on September 27, Starbird, in an August 24 message to the labs, noted the possibility that a specific definition of a nuclear explosion might be inserted in treaties as they would be tabled, and requested both laboratories to comment on possible definitions and how they would relate to treaty language. Harold Brown replied that a one- or five-ton limit on an explosion would be appropriate, although he didn't feel there was any particular difference between the two numbers as to whether the Russians would accept those yields in a definition. Norris Bradbury, on the other hand, felt that the number shouldn't be anything like that high since, if he were the Russians, he would scream bitterly about limits in the ton area on the basis that it was just a guise for developing tactical weapons. Bradbury felt the definition should not include any limit higher than about ten pounds. However, he added that as to its purpose and effects in various countries based on whether they had open or closed societies, he felt that about the only thing a limit in the definition would do would be to provide a basis for internal instructions, i.e., in the United States, the weapons laboratories would probably be directed to stay within the terms of the definition, whereas in Russia, it is not clear at all that that would be the case.

Chairman McCone made some interesting comments about the international situation and the test ban discussions to the Commission staff in a briefing on August 25, 1960. Part of a summary of his discussion on international activities stated that:

> Test cessation negotiations have been recessed until September 27 as a result of his (McCone's) trip to London, at the request of the President, accompanied by Undersecretary of State Livingston Merchant, Mr. Philip Farley, and General Starbird. The visit afforded an opportunity to discuss with the British the various questions separating the U.S.-U.K. on the one hand, and the Soviets on the other, and to explain to the U.K. the difficulties involved in a unilateral offer to disclose devices used in seismic improvement work. The discussions identified about 12 areas of disagreement with the Soviets and, importantly, dispelled the impression that if agreement could be reached on site inspection, all other points of disagreement would be resolved. It was agreed to recess to let some of the issues become quiet and to review again our own position. Mr. McCone said he was anxious that the test suspension not become an issue in the election campaign because many of the issues were not clearly understood and the whole matter had assumed an importance quite apart from other aspects of our military stature. Competing campaign promises would prevent an objective analysis of the situation and might force us into an undesirable position in future negotiations.

However, the former commissioner, Thomas F. Murray, would not permit this subject to be ignored in the Presidential campaign of 1960. On September 6, he addressed an open letter to Vice-President Nixon and Senator John Kennedy, discussing the U.S. position at the test ban negotiations in Geneva and noting "the grave threat which our current nuclear test policy poses to the national security of the United States and that of our free world allies." After review of the issues of the current test ban situation, he expressed confidence that the two candidates would recognize the validity of:

> . . . the following proposal: (1) that the present ban on atmospheric tests should be retained; (2) that the ban on underground tests and on tests in outer space should be immediately revoked; (3) that these tests should be conducted not merely to enlarge our scientific knowledge of seismic or outer space phenomena but also and explicitly to develop the technology of nuclear weapons.

Kennedy responded first in a letter to Murray released on October 10 which gave a general outline of what he would do about these matters if elected President. First of all, he stated that the U.S. would not be the first to resume testing in the atmosphere. Addressing the subject of the Geneva Conference, he noted that if it were to terminate before the inauguration, he would immediately (after inauguration) invite Britian, Russia, and France to participate in a new conference for the same purpose. Thus, whether the talks had ended or not, he would intend either to continue or reopen them. He stated the following as his feelings about how they should be continued and on what time scale:

> I intend to prescribe a reasonable but definite time limit within which to determine whether significant progress is being made. <u>At the beginning of the period, I would direct the Atomic Energy Commission to proceed with preliminary preparations for underground tests</u> of the type in which radioactive substances would be forever sealed within the explosive cavity. If, within the period, the Russians remain unwilling to accept a realistic and effective agreement, then the world will know who is to blame. The prompt resumption of underground tests to develop peaceful uses of atomic energy, research in the field of seismic technology, and improvement of nuclear weapons should then be considered, as may appear appropriate in the situation then existing. (Emphasis added.)

Nixon gave his reply to Murray's letter in a speech delivered at Toledo, Ohio, on October 26. The following are extracts from Nixon's speech:

The security of the U.S., and of the entire free world, simply will not permit either such a surrender (referring to lack of adequate inspection and control) or the indefinite continuation of the present moratorium, entirely without inspection. . . . The time and patience which we have already expended to explore this way out of the disarmament dilemma have been fulsome proof of our own intentions and of the Soviets'. The blame rests squarely on them. We cannot permit further delay. . . . Another delay of the length indicated in Senator Kennedy's proposals could be decisive in a struggle for peace and freedom. . . . If I am elected, I will, on November 9 ask the President to designate Ambassador Lodge to go to Geneva personally to participate in the present negotiations with a view to resolving this question by February 1. . . . I would have Mr. Khrushchev know that if Ambassador Lodge and the Soviet negotiator are able to bring an agreement in sight in this 80-day period, I would be prepared to meet with Prime Minister Macmillan and--so important do I hold this question to be--with Mr. Khrushchev to make the final agreement at the summit. But I would have him understand that if at the end of the 80-day period--by February 1--there is no progress, the United States will be prepared to detonate atomic devices necessary to advance our peaceful technology. Such devices already are prepared for underground use in such a way as to guarantee no contamination. Further, I would have him understand that the United States is willing to continue negotiations for a nuclear weapons test ban as long as the Soviet representative will sit, but not under an uninspected moratorium of indefinite duration. I would have Mr. Khrushchev understand that if an agreement is not signed within a reasonable period after February 1, the United States will have no alternative but to resume underground testing of atomic weapons. I say underground testing because there is no question of resuming tests in the atmosphere, where some still undetermined danger of contamination exists. The United States has abandoned such testing, certainly until more knowledge is available as to the exact consequences.

To Murray, these two responses seemed not to recognize the gravity of the situation and the need for specific and immediate test resumption. Accordingly, he wrote a second open letter on November 4 adding what he felt was a very strong reason for getting back to the business of weapons development unhampered by a test moratorium. This was to him the necessary step that must be taken to develop what he called a "third generation" of weapons (after the fission and fusion devices), referring to enhanced radiation weapons and, specifically, the "neutron bomb." Murray stated that "the necessity for negotiations is no excuse for delay in resumption of the tests necessary to put us in possession of third generation weapons. The question of who will first get these weapons is the new form of the old question of survival." Although this letter apparently led to no further statements by either candidate, a number of scientists reacted negatively to Murray's statements about the "neutron bomb," among which were Hans Bethe and Jerome Wiesner, soon to become President Kennedy's Scientific Advisor.

Given below are a few extracts from weekly status reports written by Dr. Carl Walske, the AEC's representative to the Geneva diplomatic delegation through all of 1960 and the first part of 1961. Walske's comments, with the date on which they appeared in his weekly report to the Director of the Office of Special Projects at Headquarters, AEC, give some interesting insight into how an observer sitting in on the conference meetings viewed the whole situation after the May 1960 summit conference:

June 3: I do not know what the new hard Soviet line means and can hardly guess. It looks to me, though, like the treaty here is going to have slow going for some time to come.

June 17: One might say that the events of this week are merely a simple repetition of the ills that this conference has known for a long, long time. The fact that the proposed research program detonations are in an almost hopeless mire, and that we are stalemated on almost every outstanding major issue the conference has left, are in a way old news. Still, here in Geneva, I can couple these events to a visible Soviet inflexibility and, in fact, a certain indifference on their part to whether the conference makes progress or not. . . . It has always been the case the U.S. needed to be more decisive in either getting out of or getting into this negotiation. We always said, too, that time worked against us and the longer we waited, the harder it would be to drive a good bargain with the Soviets. My personal feeling is that we have already waited too long, and while it is still possible to decide that we must settle for whatever treaty we can get, it looks like a poor treaty is all that we can now expect.

July 1: In Geneva this has been "Save the Conference Week." After the Soviet bloc walked out of the Ten-Nation Conference this last Monday (June 26), the British developed a bad case of nerves, with Sir Michael and Ormsby-Gore terribly worried that the Soviets would break off our negotiations also. . . . In a prepared statement, Tsarapkin announced that the Soviets would not send observers to our July 12 Plowshare chemical explosion. He stated flatly that there could be no Western observers at future U.S.S.R. industrial explosions. In answer to his claim that observation at industrial chemical explosions is an inappropriate subject, Wadsworth replied that the present test ban negotiations had made such observations singularly appropriate. Tuesday (June 27) saw a continuation of the British nervousness over the negotiations. In the morning, Sir Michael met with Wadsworth to convey his great concern that the Soviets were in an excellent propaganda position to break off our talks. He argued that if they walked out now, they could declare that they would not be the first to resume nuclear testing, thereby placing the West in a very hard position. (Reference is to the Ten-Nation Committee on disarmament which met at Geneva beginning on March 15, 1960.)

August 5: For the last two days, the delegation has been in a state of shock arising out of an August 4 *New York Times* story by John W. Finney. This story reported consternation of officials in the Department of State and the Atomic Energy Commission on the delegation's recent tactics in handling the safeguards issue. The feeling of all members of the delegation is that while there is room for sincere differences of opinion on tactics it is nevertheless disastrous for stories such as this to appear. The feeling is that this story may well lead to the discrediting of the present negotiating team and to the projection of the issues of the conference into the present political campaign. . . . The direction I would have liked to have seen the delegation take on safeguards was somewhat different than that actually taken. I had a chance to put forth that idea to the whole delegation. I thought that we should vigorously continue the argument for our original pool idea. The State Department element felt, however, that we should not overextend ourselves in that one direction if we were in fact likely to fall back at a later time. While I did not agree with this, I think that it is certainly a matter of opinion, and there is no doubt that they are the bosses here in Geneva.

August 12: We hear rumblings of the interagency battle on safeguards clear over here in Geneva. We are all most anxious to see how it all comes out. . . . In the briefest of terms, my view is that it is no longer possible (if it ever was) to obtain Soviet agreement to a treaty with adequate safeguards. This belief is predicated on my estimates that the Soviets will stop far short of agreement on the measures necessary to 'shore-up' the control system.

October 14: It is to the regret of all elements of the delegation that our negotiation is merely treading water. The U.S. seems to be waiting for the proper moment before deciding whether it will accept a technically imperfect treaty or whether it prefers to have no treaty at all. In our discussions here we have been talking about ways in which the day of decision might be speeded up. However, in view of the recent Kennedy statement to the effect that, if elected, he would like to take his own look at the negotiating situation, we are not hopeful. In the event of a Nixon victory in the elections perhaps we could hope for action soon after November 8.

November 3: He (Ambassador Stelle) pointed out that both candidates for the Presidency have indicated that the U.S. should make one more effort to break the deadlock in our conference. Stelle said that in his opinion, the U.S. would be remiss in its duty if, upon failure of such a last effort, it continued the *de facto* moratorium. He was confident, he said, that either candidate as President would not relax the U.S. insistence on effective controls. . . . Yesterday, Tsarapkin gave a fuller response to Stelle's speech on Monday . . . He interpreted Stelle's remarks about the feelings of the Presidential candidates as indicating the U.S. foresees the failure of the talks and the renewal of tests. He claimed further that our Monday statement indicated that the U.S. wants everything its own way. . . . Stelle, in his reply . . . said that the U.S. does want a treaty with adequate controls, however, continued negotiations without control and a treaty causes increasing concern in the

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U.S. Particularly to the officials who are primarily charged with our defense, the continued absence of testing without accompanying controls is distressing. Stelle countered a Tsarapkin remark--to the effect that our moratorium position is aimed at resumed testing--by pointing out that we are in fact now free to resume tests upon prior announcement of our plans. The moratorium was agreed to by the U.S. and the U.K. in response to a Soviet request. A moratorium implies a certain definite time at the end of which freedom of judgment will be possible. If the Soviet attitude is that this is not the case, then the treaty is in effect comprehensive, and the Soviet acceptance of a threshold treaty is hollow, said Stelle.

November 10: We have now heard the results of the election and are trying to guess what the future will hold. If, as we expect, it will not be possible to get major decisions on this conference until after the inauguration, then we feel a rather long recess would be best. Under the circumstances, such a recess would enable the U.S. to maintain the best possible posture until it was once again ready to negotiate actively. At the moment, it would seem appropriate to begin a recess in a couple of weeks, and to have it last until early February.

Following the recess in December, the Chairman notified the Commission at a meeting on December 19 that they had received a Presidential request to join with the Departments of State and Defense in preparing a coordinated position paper on the subject of nuclear testing. In the same month, McCone received a letter from Herbert Loper, the Assistant to the Secretary of Defense for Atomic Energy on the subject of discussing possible benefits of nuclear testing.

Summary of 1960

At the end of the year 1960, test ban negotiations were essentially at a stalemate, partly because of U.S. insistence on the various aspects of underground test safeguards and the Russian reluctance to accept our view, and partly because of the personal vendetta between Eisenhower and Khrushchev, who intended to await a new U.S. administration before agreeing to further moves. The French had entered the nuclear community by conducting three tests. President-elect Kennedy, in pre-election statements, had made it clear that he intended to break the deadlock in some fashion, insisting on "adequate controls," and had stated that he would direct the AEC to begin preliminary preparations for testing but not in the atmosphere. The overall governmental system had conducted a review of the value to the country of further nuclear weapons testing without arriving at any particular conclusion. The Atomic Energy Commission and it's General Advisory Committee seem to have convinced themselves that the odds were fairly high that the Russians were testing clandestinely. With respect to our ability to go back to nuclear weapons testing, the national philosophy that any future testing would be either underground or in deep space had hardened and, hence, our capability to test in the Pacific had been degraded so as to become almost miniscule. The Eniwetok Proving Ground had been turned over to the Pacific Missile Range and the Task Force and Task Groups either inactivated or degraded strongly. The likelihood of Army installation of missile launchers on Johnston Island that could be used for Willow had been reduced to almost zero and all work on Willow had been stopped. However, in Nevada, an appreciable amount of work had been done during the year to produce tunnel sites for detonations either under the auspices of readiness in the early part of the year, or Vela Underground later. The Nevada organization had found a great deal of work to do, not only on the above subjects but also on Plowshare. For similar reasons, the AEC Laboratories' testing capability, in one way or another, had been preserved either by conscious readiness effort or by transfer of people to related efforts such as Vela Uniform, Plowshare, Rover, Pluto and special laboratory experiments. In some fields that would be important in future weapons testing, effort had actually increased. At LASL, there were many new people involved in Vela Hotel and Vela Sierra who would later use this know-

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ledge in conducting experiments on Dominic. The DOD, after having gone through some sort of minimum in their capability in the middle of the year, were in some ways on the way back up and had managed to continue their preparations for Jericho (or Marshmallow). AFSWC was getting increasingly deeper into the need for high-altitude detonations and the study of the phenomena. Thus, it appeared that the odds of resuming testing had increased appreciably over those at the beginning of the year, and the capability to test underground had been improved, although many of the diagnostic methods were still poorly defined.

Personnel Changes

In the early part of 1961, there were a number of personnel changes amongst the people considering nuclear weapons testing and the test ban problem. Some of the more significant of these are shown in Table XI. Spurgeon Keeny stayed on the staff in the office of the President's scientific advisor, where he had been since the formation of that group. Stelle was temporary head of the United States delegation at Geneva until Dean's appointment.

TABLE XI
KEY PERSONNEL CHANGES

Position	Incumbent/ Date of Leaving	Replacement/ Date of Arrival
President	Dwight D. Eisenhower January 1961	John F. Kennedy January 1961
President's Special Asst. for Science and Technology	James R. Killian June 1959	George B. Kistiakowsky June 1959
President's Special Asst. for Science and Technology	George B. Kistiakowsky January 1961	Jerome B. Wiesner January 1961
Secretary of Defense	Neil H. McElroy December 1959	Thomas S. Gates, Jr. December 1959
Secretary of Defense	Thomas S. Gates, Jr. January 1961	Robert S. McNamara January 1961
Deputy Secretary of Defense	Donald A. Quarles May 1959	Thomas S. Gates, Jr. June 1959
Deputy Secretary of Defense	Thomas S. Gates, Jr. December 1959	James H. Douglas, Jr. December 1959
Deputy Secretary of Defense	James H. Douglas, Jr. January 1961	Roswell L. Gilpatric January 1961
Secretary of State	John F. Dulles April 1959	Christian A. Herter April 1959
Secretary of State	Christian A. Herter January 1961	Dean Rusk January 1961
Chairman, AEC	Lewis L. Strauss June 1958	John A. McCone July 1958
Chairman, AEC	John A. McCone January 1961	Glenn T. Seaborg March 1961
U.S. Ambassador to Geneva Test Ban Talks	James J. Wadsworth September 1960	Arthur H. Dean January 1961
Advisor to the President on Disarmament	None (new position)	John J. McCloy January 1961

TABLE XI (continued)

Position	Incumbent/ Date of Leaving	Replacement/ Date of Arrival
Director, Advanced Research Projects Agency	Roy W. Johnson October 1959	Austin W. Betts February 1960
Director, Advanced Research Projects Agency	Austin W. Betts January 1961	Jack P. Ruina January 1961
Director of Defense Research and Engineering	None (new position)	Herbert F. York December 1958
Director of Defense Research and Engineering	Herbert F. York 1961	Harold Brown May 1961
Asst. to the Secretary of Defense (Atomic Energy)	Herbert B. Loper 1961	Gerald W. Johnson August 1961
Chief, Defense Atomic Support Agency	Edward N. Parker August 1960	Robert H. Booth[a] January 1961
Director of Military Applications (AEC)	Alfred D. Starbird January 1961	Austin W. Betts January 1961
General Manager, AEC	Kenneth E. Fields June 1958	Paul F. Foster July 1958
General Manager, AEC	Paul F. Foster November 1958	Alvin R. Luedecke December 1958
Director, Lawrence Radiation Laboratory, Livermore, CA	Herbert F. York March 1958	Edward Teller April 1958
Director, Lawrence Radiation Laboratory, Livermore, CA	Edward Teller June 1960	Harold Brown July 1960
Director, Lawrence Radiation Laboratory, Livermore, CA	Harold Brown March 1961	John S. Foster, Jr. June 1961 (acting: March 28-May 31)
President, Sandia	Julius P. Molnar August 1960	Siegmund P. Schwartz September 1960
Commander, Field Command, DASA	Louis T. Heath June 1960	Harold C. Donnelly[a] June 1960

[a]Donnelly acted also as Chief, DASA, from August 1960 to January 1961.

Growth of Readiness Interest, Early 1961

The change in administration as a result of Kennedy's election in November 1960 led to renewed consideration by the Commission and other portions of the government of the test ban treaty negotiations and the wisdom of continuing the moratorium. On January 13, Starbird sent to McCone a long report on the possible benefits of nuclear testing, emphasizing some of Loper's points made slightly earlier, and saying, among other things:

> I believe these ideas constitute a much more powerful support of a position that we must resume testing . . . Only by increasing the capability of our present state of the art by factors of two, three, or more can we

expect to deliver the yield which the DOD now estimates necessary for targeting purposes . . .If, through testing, it is possible to go the other way and through better warheads actually reduce the number of missiles, aircraft, logistics, support equipment, and men required, the net savings in better defense posture would indeed be spectacular.

On January 18, McCone stated before the Joint Committee:

> Further attempts to reach agreement with the Soviets should be made promptly. If an agreement is reached promptly, the United States, under safeguards, it is proposed, and in coordination with the U.K. and the U.S.S.R., should pursue vigorously the development of improved techniques and equipment for use in a control system on which an agreement would depend. If, however, the Soviets attempt to continue indefinitely the present unpoliced moratorium by prolonging the negotiations, the Commission believes that there is only one prudent course for the U.S. to follow. This course is a resumption of testing of nuclear weapons underground and possibly in space.

On January 18, Harold Brown wrote to Shute (San Francisco Operations Office), giving some of his thoughts on the subject of the requirement for testing:

> Very much more elaborate techniques of calculations and nuclear weapons design, along with a certain amount of laboratory experimentation, has [sic] served as a partial substitute for weapons tests. The result has been very real and important but considerably diminished progress in weapons design and development. If testing is not resumed, we expect, in the period through FY 1963, to proceed with work on such items as a pure thermonuclear explosion, variable diameter warhead [redacted] and so forth on which some progress can be accomplished without test. . .The exigencies of the seismic improvement program have essentially eliminated the readiness capability for resumption on short notice of full-scale underground testing . . . If such testing is resumed, we would be in a position to test (and stockpile if successful) a variety of items [redacted]

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He concluded that the test moratorium had considerably impeded weapons development, and that while some progress could be made without full scale nuclear testing, great care had to be exercised in stockpiling new designs.

On January 25, 1961, the President appointed a disarmament study group headed by Dr. Fisk and reporting to John J. McCloy, the President's Disarmament Advisor. General Starbird, General Betts, Spofford English, and others from the AEC staff were participating members of this study group. Among the studies to be made were the probable gains to the United States from various types of nuclear tests, probable gains to the U.S.S.R. from various types of nuclear tests, and a comparison of the relative gains to each side in terms of improved weapons systems. At the January 28th meeting of the Commission, Carson Mark, pointing out the difference between his evaluation and that of Harold Brown of Livermore, stated that he was "essentially pessimistic about gains which might be made from additional testing." [redacted]

At the February 1, 1961, Commission meeting General Betts, who had just taken over from Starbird as head of DMA, in a briefing for Chairman-Designate Seaborg, said a major problem faced by the Disarmament Study Group (Fisk) was to determine the validity of weapon capability projections in the absence of testing. [redacted] He offered the opinion that the Russians might find it possible to develop weapons in the megaton range by clandestine testing. As to developments in

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our own future, he pointed out that the U.S. was engaged in the development of enhanced radiation weapons, noting that such weapons would be especially effective in inflicting casualties upon personnel in foxholes and tanks while minimizing fallout. [redacted]

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The uncertain future of weapons tests in political circles was evinced in *Headline Series #145*, published by the Foreign Policy Association of the World Affairs Center and entitled, "The Future of Nuclear Tests." Teller and Bethe, in this January-February 1961 issue, published opposing viewpoints on continued nuclear testing. Bethe argued that there was little to gain from further testing and little risk of the Russians catching up with us under a complete test ban. Teller, on the other hand, argued that testing would allow the development of an effective "second strike force" and of small tactical bombs to increase our options in the realm of limited warfare. He also gave Plowshare as a reason for continuing testing. He argued that the moratorium had been seen by some as a first step toward relaxation in further arms control, but that, obviously, there hadn't been relaxation in arms control, but rather increased intensity in the cold war. There was no notable reaction to this publication.

On February 16 the Commission briefed the new President on nuclear weapons subjects, hoping to get some indication of his intent. He does not seem to have committed himself in any way, but did request the Commissioners' judgment on the effects of a continuation of the test ban through June of 1961. The various Commissioners answered that there was no particular concern about another six months extension of the test ban, but that there was a real concern if it were to continue much longer.

The Fisk panel continued its work through February and March, with some notable nervousness on the part of the Commission. On March 1 Betts reported to the Commission [redacted]

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Bradbury, in his April 13, 1961, program letter to DMA, commented:

> Military requirements continue to appear for new nuclear weapons in spite of the moratorium on full-scale testing which has existed for the past 30 months. These demands, coupled with the Presidential directive to the Laboratory to maximise its progress in nuclear weapon development within the restriction of no nuclear testing have materially increased the theoretical and experimental effort which is required to assure ourselves and others that new weapons introduced into the stockpile will be certain to perform approximately as expected.

He went on:

> The Laboratory is conducting an extensive theoretical and experimental program whose ultimate objective is to provide a common basis of fundamental data and calculational procedures which will bring together in an understandable whole all of the results of previous nuclear testing. It is clear that there is available to us an enormous quantity of valuable experimental information from previous nuclear testing. This information, if it could all be reduced to a common calculational basis, would obviously provide the strongest support for the firm prediction of performance of new weapons with thorough confidence in their behavior. Furthermore, the Laboratory is aware that there has been a steadily increasing emphasis on the nuclear safety of atomic weapons and, indeed, this feature now dominates the design of all primaries.

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In April, Bradbury, as a member of the USDA* "Disarmament Consultative Group," discussed for the Chairman, Harvey Brooks, an interesting proposal:

> It is possible that the intransigence of the U.S.S.R. in the test ban negotiations is due to the fact that they do not regard the game as worth the candle. It is perfectly obvious that the current difficulties over inspection procedures, vetoes, and so on would, if extended into a disarmament situation, make it completely unworkable. Some real step toward disarmament might seem to them worth more effort.

He then discussed the possibilities of an agreement to stop producing fissionable weapon material for weapons use or, later on, stopping the production of fissionable material completely. He commented that he could see no way to verify that fissionable material was not being produced for weapons in a situation in which production for other purposes was allowed. He further commented that an agreement to stop production was not entirely incompatible with the continuation of nuclear weapons testing since the production agreement might be Phase I of a multiphase agreement. He suggested that it would be folly to discuss the details of Phases II or III before entering into the first phase, which "will certainly be an educational process." He felt that a stoppage of production would not hurt us unduly, apparently because from a strategic and national deterrent standpoint, we already have plenty of material, and he also felt that there were no strong reasons to enter into development of a large tactical device inventory, which, of course, would require vast amounts of new fissionable material. Carson Mark commented (to Bradbury) that we could perhaps accept less assurance of our control and inspection if that circumstance were coupled with a reasonably well-assured disarmament system that would stop the material production in the other country. He added:

> In my opinion, of course, we (the United States) have somewhat overdone things in our insistence on having a high degree of assurance that even rather unlikely things were not going on. This attitude has gained considerable support from the frequent repetition of rather exaggerated estimates of the gains likely to be realized from testing accompanied by, if anything, even more exaggerated estimates of the case in which tests could be conducted illegally and the certainty with which the fully developed fruit would fall neatly into the hand of whomever might jostle the tree, however imperceptibly.

On April 28, at the 300th meeting of the Geneva talks, Dean commented that, "To me it seems much more likely that within some reasonable period, our fate will have been determined and our success or failure written down upon the pages of history." Tsarapkin claimed the West wanted to wreck the negotiations and shift the blame to the other side.

During this period other detailed discussions were taking place. On January 25, 1961, Starbird answered Reeves' November request for guidance:

> In spite of . . . budget restrictions, there are desirable actions in the line of planning which might lead to a more complete test capability . . . Problem areas may be revealed which by anticipation may be more quickly and easily overcome. . . . By revealing positive and indisputable restrictions to possible future test programs . . . we may be in a better position to request budget relief for such purposes or use monies available later in the year from programs which underrun. . . Any test effort in NTS which might be accomplished within the next few years can be expected to be underground. . . . Initially, the yields would be restricted to relatively low levels, perhaps on the order of 50 kt maximum.

*U.S. Disarmament Administration.

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He suggested that planning assume a very few high priority tests commencing quickly after a decision to proceed, and being completed in perhaps three to six months from go-ahead. "Such a program might be along the lines of an abbreviated Succotash of the order of six tests, and should involve experiments from both laboratories which urgently require testing because of existing and critical weapons requirements." He also suggested a second program to be accomplished in perhaps a year from go-ahead that would have the Laboratories testing when and as required, beginning with the most promising new ideas in developments. He cautioned: "I would like to emphasize, however, that no impression should be conveyed or implied that a resumption of testing is imminent." He directed Reeves to ask LRL to redefine the Succotash program and to also approach LASL, DASA, Division of Biology and Medicine, and others with the thought of producing an overall plan which could then be examined by DMA with the idea of authorizing such portions of the new plan as might be feasible, considering budget restrictions and political implications.

The Labs responded leisurely, with Graves commenting that it was worthwhile to have some general plan of what weapons tests should have first priority, but that he couldn't see the likelihood of major construction effort, and, furthermore, he would in general rather see planning done on a less formal basis rather than trying to develop a single plan or a set of alternate formal plans. He commented in mid-February that LASL could organize any test effort within the time that would be required for the Lab to prepare the test devices for use. At the March 2 LASL Weapons Working Group (WWG) meeting, Graves noted that with the Geneva talks starting later in the month, and in their present status, the end of the moratorium would probably be abrupt, and he raised the questions of the wisdom of a readiness program and production of appropriate devices. The feelings of the Laboratories were illustrated at a March 17 meeting at Livermore, attended by members of all three Laboratories, at which Bradbury commented that he didn't understand why there should be great urgency to resume testing immediately after receipt of permission since it did not seem likely that testing, once resumed, would soon again be subject to pressures to stop. On the other hand, Harold Brown of Livermore expressed urgency to develop a readiness program because by reducing the present six months readiness to approximately two months, the urgency of any future decisions regarding nuclear testing would be increased and the possibility of using a long lead time as an excuse to delay decision would be eliminated. Brown commented that LRL would send a letter to DMA in the near future which would outline an up-to-date readiness program. Reeves then requested a joint meeting in Albuquerque on April 20 to lay the groundwork for a test readiness program.

True to his word, on April 10, Harold Brown proposed to General Betts (DMA) a new LRL test readiness plan. [redacted] Overall Nevada costs would be just short of $2,000,000. Brown requested that Betts authorize the program. Bradbury apparently did not follow a similar path; however, the level of LASL in-house discussion on the subject grew. On May 3, in order to get a little further along with the definitive program, the discussion in the LASL WWG was continued, with Harold Agnew noting some interesting ground rules:

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> The AEC wishes the Laboratories to be able to start on short notice, i.e., within three weeks to a month, simultaneously stating that no advance preparations will be made at NTS. . . . Surface contamination from the underground shots is to be contained within the test site. . . . It is believed that something like 1 kt is possible in the 450-foot holes now available to LASL, but that this is probably the upper limit for at least the first few unless some of the LRL facilities are made available to us. Ogle noted that if alpha measurements are desired, then two to three months would be required for the first test whether the holes are available or not.

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A tentative list of possible shots was given, [redacted]

On April 13, in a report to DMA on overall LASL activities, Bradbury commented:

> The Laboratory is not making any specific plans for an immediate resumption of testing in the event the moratorium is abandoned. Since the circumstances under which this could occur and the restrictions which might be placed on testing in any event are so varied, detailed planning seems futile. Furthermore, if the Geneva negotiations are abandoned, and testing resumed, it is not easy to see what combination of political circumstances would suggest it be important to start testing very quickly, or could lead to its early abandonment again. Nevertheless, the Laboratory will rather rapidly acquire a number of systems in which it would be interested for testing.... Clearly, what actually would be planned would depend to a very large degree on what we are allowed to do--and this, of course, remains to be seen.

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Reeves' meeting was finally held on May 10, 1961, with representatives from LASL (Graves), LRL (Bacigalupi), EG&G (John Lusk), H&N (Hal Perla), Field Command (Carse and Tate), Las Vegas Area Office (Yelinek), and others. Graves and Bacigalupi outlined the available LASL and LRL facilities and suggested a strawman distribution using some of the Vela Uniform sites. [redacted] The discussion led to the suggestion that LASL use the e.03 tunnel complex as well as the Area 15 tunnels, including 15a, the 950-foot deep, 36-inch granite hole (Lollipop-High Hat). It was agreed that DMA would be asked for guidance on the question of planning to use Vela Uniform sites. EG&G reported that they had approximately four weeks readiness for timing and firing, using some five minimal timing systems available either at NTS or elsewhere, and five tunnel-type zero racks which could be adapted to almost any use. Assuming they promptly purchased an inventory of film, they had a similar readiness for photography. Because of the previous LRL commitments for Plowshare, EG&G could put together a system of 32 oscilloscopes for LRL alpha measurements, for which they had about six weeks readiness, depending upon the test area. They (EG&G) were committed to LASL for a 15-scope system to be ready by July 1 for use on Vela shots, and there was a 27-scope alpha system at LASL which was committed. Bacigalupi noted that an initial survey of the cable available showed enough for the proposed LRL shots, but that if LASL were to use any of the tunnels, there then might not be enough cable. EG&G noted that for LASL there were only 10 alpha detectors available, which would be enough for two shots. The lead time on detectors could be as long as 130 days because of photocell procurement. Bacigalupi noted that seven e tunnel sites were available, the yield capability running from [redacted] and that four b tunnel sites were available with yield capability [redacted] The yield capability was based on the 450 times the cube root of yield rule for containment and a 600 times the cube root of yield estimate to preclude damage to adjacent tunnels. The meeting ended with an agreement that both Labs would make a firm outline of a proposed program for planning purposes and recommend sites for the location of the events.

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Considerations were also taking place in the DOD during this period. In early January, General Schriever, Commander of Air Force Systems Command, in preparation for impending congressional hearings, asked AFSWC to prepare a study on their needs

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were testing to be resumed. The initial reply on January 18 stated AFSWC's belief that nuclear tests would not again be conducted in the atmosphere, but emphasized the need for high-altitude or space tests and even suggested attempting some of the high-altitude phenomenology underground. AFSWC in-house studies (Thayer and Eddy to McCorkle) mentioned other items that could not be studied properly without further testing; x-ray phenomena, including output and lethality questions; turbulent Argus, which was the effect of radio noise produced from high-altitude events; the space and time extent of blackout effects; further information on the transient radiation effect on electronics (TREES); the experimental verification of neutron and gamma transport codes from airbursts; and tests to look at [redacted] etc., for Project Orion (which the DOD called Project Putt-Putt). It was expected that most of these would be very difficult to investigate by underground testing. In mid-February, the weapons effects board of DASA recommended an increase in the AFSWC budget, apparently especially for increased effort in high-altitude effects and underground protective construction. Herb York, now DDR&E, stirred up further interest with the comment to Booth on February 22: "It is expected that during the next six weeks, a decision will be made which will further determine national policy in the matter of weapon testing." Booth, on March 15, offered guidance to Field Command, including continued direction of the Marshmallow (Jericho) program, which presumably had a 12-month readiness, commenting that he was considering actions to reduce lead times, provided the costs were not prohibitive. Furthermore, he directed them to "discontinue planning for inclusion of military weapons effects tests in the Vela Uniform explosion program" (presumably speaking of Lollipop) and "within current personnel and fiscal ceilings continue to maintain a capability to resume nuclear tests." At a briefing for General Schriever given by AFSWC in mid-February, some of the penalties of not testing for the last two and one-half years were noted. For example:

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[redacted]

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In spite of the thinking that any future testing would only be underground or in deep space, and perhaps simply as a matter of inertia, the MLC noted in early April that the Air Force had retained a capability to provide sampling support to the AEC in the event nuclear testing was resumed, and asked for a review, in the light of the present situation, of future AEC sample support requirements. Betts transmitted the request to the Labs on the assumption that a 6- to 12-month buildup period would be required, but he also asked the Laboratories to give their judgment on the buildup time. After the appropriate inputs from the Laboratories, Betts replied to Major General Bruce Holloway at Headquarters U.S. Air Force in early May that some six sampling aircraft with appropriate spares would be the minimum for overseas tests and four for Nevada tests. If drones or rockets were developed and proven, the aircraft requirement could be reduced or even eliminated. Betts also added that if a situation should arise which would dictate atmospheric testing:

> A buildup period of 6 to 12 months testing at NTS is realistic. We would assume that a longer period, perhaps 18 to 24 months, would be required for comparable tests overseas, although a simplified test might be conducted in somewhat less time. . . . For a possible underground sustained testing series at the NTS, a 6- to 12-month

> preparatory period is generally applicable. We would be prepared, however, to conduct a limited number of simple experiments such as safety tests or low-yield proof tests within one to three months should circumstances demand.

In mid-April 1961, Colonel Byrne (4950th Test Group), because of informal discussions with various personnel closely associated with various phases of work on atomic energy, observed that "the Geneva talks may culminate in the resumption of testing prior to the end of CY 1961." Still trying to carry on his job of providing air support for nuclear weapons tests, he rather plaintively requested that AFSWC headquarters provide information on the current test planning. To our knowledge, Byrne was never answered.

Demise of JTF-7

Except for the Air Force, the overseas test organization gradually disappeared in early 1961. As had been planned in mid-1960, the Task Force Headquarters had been reduced to a planning element within DASA by February 1, 1961. During January, 22 out of the last 24 people in the headquarters were transferred out of JTF-7, with Colonel Thomas L. Mann and Commander Frederick E. Bitting remaining. Thus, by March 1, JTF-7 had become an integral part of DASA.

The JTF-7 Draft* "Operations and Reactivation Manual" of March 1961 notes that after the report of the "Study Group on the Organization for Future Test Operations" was accepted for implementation in the fall of 1959, certain difficulties were noted. Specifically it did not seem feasible to maintain the required twelve month readiness status. As a result another study, the Reappraisal of Requirements for the Eniwetok Proving Ground and the Readiness Status and Functions of JTF-7, was conducted. A part of it states:

> Study of the effect of the disestablishment of the Task Force led to the conclusion that this was too drastic a step. Instead, it was determined that deactivation was a more proper attitude with the Task Force remaining in existence less personnel and material. In this way, the Task Force "know-how" could be maintained by keeping a live file of plans, records, instructions, and pertinent information in an available location. A capability to keep abreast of current thinking would be maintained and broad, flexible plans kept up to date. If, at any time in the future, a decision was made to activate a joint task force, the information on which to build would be available and plans would be in existence that required only details and further updating to bring into full use.

In spite of the above reasoning, Joint Task Force 7 was discontinued on June 30, 1961. Colonel Mann remained in DASA. On June 30, 1961, pursuant to JCS Order SN928-60, dated September 30, 1960, DASA issued orders for the discontinuance of JTF-7 effective June 30, 1961.

The AEC Support Task Group 7.5 had been discontinued on December 1, 1960, and 7.2, the Army Task Group, was discontinued by order of JTF-7 on January 24, 1961. 7.2 had been simply a paper organization since 1960, with no personnel or equipment. 7.3, the Navy Task Group, had been relieved from assignment to JTF-7 and assigned to Headquarters DASA on March 22, 1960, by JCS authority. On January 11, 1961, it was transferred back to JTF-7, where it apparently remained until the disestablishment of JTF-7 on 30 June 1961. Tom Mann, the last commander of JTF-7, was reassigned to

*DASA-70-03346.

DASA, and lasted there long enough to assist in establishing JTF-8, joining it in 1962.

The odd existence of 7.4 continued in its uncertain fashion. It was never really clear whether the 4950th was 7.4 or not, but they continued to wear that hat through part of 1961. Apparently triggered by conversations with Kenner Hertford, Colonel Byrne of the 4950th prepared in February a "fill in the blanks yourself" skeletal operation plan for a future open seas nuclear test operation, which was released on March 27 as a Task Group 7.4 document. It commented that ALOO had recommended to the AEC (early in the moratorium) the resumption of nuclear weapons testing at an early [redacted]

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Byrne managed to keep samplers operating not only for Rover missions, but also, in coordination with AFTAC, with flights out of East Sale Air Base, Australia, in May and June 1961. The discussion about the existence of the 4950th and the proper place for the 4926th sampling group continued. On May 1, 1961, McCorkle, commander of AFSWC, wrote to Schriever, Commander of Air Force Systems Command, among other things, "Most Air Force requirements for nuclear testing could be satisfied with underground and space tests, and political considerations would dictate that large-scale atmospheric tests such as Hardtack and Redwing would probably not take place." Speaking of the 4950th planning function, McCorkle added:

> The function now becomes one of realistic planning based on the nature and scope of any future testing and the scientific parameters that would be related to such testing. Test plans should be prepared and maintained based more objectively on envisioned scientific requirements related to space and underground testing rather than on a contingency support. . . . Plans for future tests should include active support that would be required from all AFSWC agencies, such as space vehicles, instrumentation, satellites, space hardware, launch crews, and the like.

He recommended that the 4950th be deactivated, that the 4926th (Sampling) be reassigned within AFSWC, and that a test planning office be established within his own headquarters, taking over most of the spaces remaining in the 4950th. Some of the slots from the 4950th would be used to man the Nuclear Warfare Laboratory (which was under construction) and to establish an active Nuclear Reactor Safety program.

Colonel Byrne, however, continued to struggle, and on July 20 he pointed out that for the first time since September 1960, Field Command DASA was now allowed to design nuclear test experiments in environments other than underground or space, and that the Rover and Pluto efforts were growing, with NASA planning to put approximately 1,000 people in residence at the Nevada Test Site. He speculated that while testing might begin underground and in deep space, "with the restrictions having been lifted on planning for tests in the atmosphere, both atmospheric and underwater tests would also occur." He added:

> The current belief of Field Command, DASA, is that DASA will be given the responsibility which will further be delegated to Field Command to be the focal point of all planning and executive agent for the DOD responsibilities to nuclear test activities. . . . Such an organisation or organisations would place Field Command, DASA, in the same relation to the conduct of all types of nuclear testing activities as Joint Task Force 7 was placed in regard to overseas weapons tests.

In this situation, he suggested that DASA would rather "have an Air Force organization speak to other Air Force organizations in support matters rather than a bluesuited member of a joint command performing this activity" and suggested, therefore, that DASA would prefer to continue the present arrangement, since the 4950th had had this function since 1956. Nevertheless, in August 1961, the 4926th was transferred to the Air Weather Service on the basis that most of their missions were for U.S. weather programs, and the 4950th went out of existence. The Laboratories, especially LASL, were most concerned about the transfer of the sampling capability to the Air Weather Service and did manage to get an agreement that the sampling mission would have the highest priority.

Vela Uniform: Black Box, Etc., 1961

In the early part of 1961, Vela Uniform, the seismic improvement program, continued to represent one of the major efforts at NTS. However, the program was essentially stuck without further decisions. The U.S. had not come to any agreement with the Russians on a joint test program, and could not make up its mind to go ahead unilaterally. The device to be used depended upon political considerations which could only be decided after further negotiations with the Russians. The Geneva negotiations had been recessed on December 5, 1960, and were not to convene again until March 1961. Furthermore, the Bureau of the Budget had indicated that funds for the program would be cut for the next fiscal year. On February 6, Betts informed the community that no decision approving the go-ahead for Vela Uniform would be forthcoming before April 1.

Nevertheless, work continued. Livermore continued preparation for their polyethylene yield determination method on the appropriate events of Shade and Dribble. The decision was made to move the Lollipop event from the 950-foot hole to a 1,500-foot hole and work was going on to locate the proper geology for such a hole. Early in the year Lollipop was scheduled for August 1, 1961. On January 19, Roger Batzel of Livermore was designated as Scientific Advisor to Reeves for Project Shade.

Construction continued on the Cottontail site (now called Linen, 5-kt high explosive) in the U-12b.07 shaft. The progress of digging was hindered by a water seepage problem, and in March, to eliminate the problem, the depth of burst was raised by about 120 feet. However, for this and other reasons, the digging schedule could not be maintained. In early April, Reeves told Betts that a seven day workweek of construction and loading of the Linen cavity would be needed to reach a readiness date before winter, as was desired for seismic reasons. The alternative was to stop work until a firm schedule was established. Betts' choice was to stop work on May 7, with the intent of establishing a firm date for the shot in the spring of 1962.

The debate concerning the device to be used for Vela Uniform continued. In early March 1961, Betts notified the Laboratories of a proposed change in the Geneva negotiations, in which we would remove the requirement for joint contribution of nuclear weapons to the pool and would offer to reveal to the original parties detailed drawings or blueprints of the devices used in conjunction with U.S. tests, including actual inspection of the device. He pointed out that this still had to be agreed to by the JCAE [redacted] Betts also asked how long it would take to provide detailed drawings suitable for display to the Soviets, as well as an exploded drawing of the Mark XI illustrating the parts of the device which could be disassembled and displayed for visual and manual examination. In that query he noted that if the Mark XI were used, hydrodynamic yield measurements would not be technically feasible, and he asked for comments on other types of yield measurements that might be done. Harold Brown promptly answered that hydrodynamic yield measurements were possible. In an 18-inch diameter hole, one could use shock time-of-

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arrival in the medium, and in a 3-foot diameter hole the polethylene-type measurement could be done. However, if the device were to be a Mark XI, then Livermore recommended alpha measurements. Reeves suggested on March 8 that if the Mark XI were to be used, LASL should take on the primary yield determination rather than Livermore, and noted that it was quite difficult to proceed with further planning for Shade until it was determined whether the shots would be Mark XIs or Mark VIIs. Bradbury consistently predicted problems with the disclosure of various blueprints or assemblies to the Soviets only, and again stated his feelings that the blueprints or drawings or both should simply be declassified rather than trying to figure out how to control the information disclosure to the appropriate people. He did not believe that the possible gain to nth countries warranted any rational concern. He wrote:

> As said before, any group that can accumulate the required amounts of material will be able to engineer an effective method of assembly without seeing our drawings just as well as after having seen them. At worst, the whole situation will provide no more than a textbook example of a classic weapon design calculation, together with the answer in the back of the book as to the yield it gives. . . . While my personal opinion is along the latter lines, I really do not care and I do not believe that it affects the national security in the slightest either way.

He went on to request more specific information on just what yields and yield measurment methods were contemplated, continuing:

> If LASL is to undertake the procurement of these devices, measurement of alpha, and other activities beyond those contemplated in our normal activities, we will need money. Nor can we guess how much until we have some idea of what the program is actually likely to be and when. How serious is this possibility anyway?

On March 21, the opening day of the Geneva session, Arthur Dean did make, as part of the Western proposal, an offer to allow Soviet inspection (assuming Congress concurred) of U.S. nuclear devices to be used for the proposed program to perfect the detection of small underground detonations. On April 4 and 10, Tsarapkin, among other things, agreed to the details of control, inspection, and monitoring of seismic research detonations.

On March 29, 1961, Schwartz of Sandia informed Betts that the total estimated costs to Sandia for the Vela Uniform program for 1962 and 1963 would be $2,600,000 for the Mark XI path and $2,000,000 for the Mark VII path.

On March 29 Betts notified the Laboratories that DMA would recommend to the Commission that only Mark XIs be used, and that DMA would also recommend relying on alpha as the primary yield measurement, supplemented wherever feasible by shock time of arrival and/or prompt sampling methods. He directed, however, that work on the Mark VII be continued in case it should be needed as a fallback position for all or some of the shots. [redacted] Two days later he informed Harold Brown and Norris Bradbury of the Commission's decision that declassifying a device and publishing blueprints of it for all to see would be extremely harmful to the United States from a political standpoint and he suggested that we not again explore the declassification possibility. In mid-April Betts requested more information about the devices for discussion with the Commission, and brought up the possible use of the Mark 23 gun device. Bradbury answered that he would not like to see the Mark 23 used [redacted]

Within LASL, there was worry about the yield measurement. Ogle pointed out to Bradbury that a satisfactory value of the yield could only be obtained from alpha

Withheld Under 5 U.S.C. 552(b)(3) Ex. 3 DOE data by going through our weapon design calculations, which were highly classified in some portions and which we would probably resist showing to the U.S.S.R.

> Thus, it seems to me that we should point out to DMA that the measurement of alpha on the seismic improvement shots will give us, the U.S., a good value of the yield, but that it cannot be considered as a yield measurement with respect to the U.S.S.R. unless we are willing to release sensitive detailed weapon design calculational methods and constants. Obviously, my point of worry is that DMA may be sold on a method that will not actually satisfy the ground rules and may tell us to prepare for such measurements. This could lead to a lot of J-Division effort to no point.

Bradbury commented that the amount of effort involved in the Mark XI program, as far as LASL could see now, would be comparable to that which was required a year ago for the Mark VII Black Boxes. On April 19, Bradbury commented to DMA that the accuracy of yield prediction for the Mark XI [redacted]

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In early April, an Orchid zero-point compatibility test was run using the Whirlaway Black Box. The LASL radiochemical sampling pipes fit reasonably well, and as far as LASL was concerned, all equipment for the Orchid test was now on hand at NTS.

Also in early April, the Commission discussed possible methods of getting the mineral rights for the Hattiesburg site, but deferred action on the subject.

In May, DASA, assuming that it would be the single U.S. point of contact for all foreign participants for both close and long-range measurements for Vela Uniform, arranged for a visit by a team of United Kingdom atomic energy personnel to Albuquerque and Hattiesburg to discuss United Kingdom participation in the Vela Uniform program. LRL participation was requested at the NTS for further visits there. The chairman of the United Kingdom planning committee was Mr. Edmond Richard Drake-Seeger. The United Kingdom projects to be discussed were in the areas of strong motion and electromagnetic measurements. In mid-May, the site selection process for Shoal had narrowed down the possible sites to the Wonderland of Rocks and Lost Valley in California, and the Sand Springs Range in Nevada (near Fallon). Reeves recommended the Nevada site to DMA, and, after coordination with ARPA, DMA agreed to that site, the final decision to be based upon further field exploration.

At Geneva, Tsarapkin had asked for more details on our proposals concerning inspection and monitoring by the Russians in our Vela Uniform activities, and Betts requested Laboratory suggestions and comments on these. Bradbury answered that we should move ahead on working out actual arrangements, and deal with difficult parts and disagreements when they came up. Hancock of ALOO suggested to Reeves, among others, the following:

> a. The U.S. would, without Soviet help, deliver the device to the test organisation at the test site.
>
> b. On-site control of the device: keys to storage locations, zero station, etc., should rest with AEC custodians and Soviet observers; seals and locks would be fixed as mutually agreed; stationing of Soviet armed guards was "unthinkable."
>
> c. Other details of Soviet coverage of intra-site device movement, access to other NTS (or whatever site) areas, limiting permissible areas and procedures for Soviet specialists setting up their instruments, and the important problem of effectively controlling the contact of Soviet with U.S. personnel would have to be considered. (The co-mingling of Soviets and U.S. personnel and assuring limited passage of information was called "a very serious hazard.")

By mid-June, neither the Commission nor Congress had agreed to the proposal of revealing the detailed drawings of the Vela Uniform device to the Russians. In mid-June, Reeves notified Betts of his forecast for the Vela Uniform detonations as follows: Orchid in the fall of 1961 (they had already had a dry run); Linen, April 15, 1962; and Crystal, June 1, 1962. Yet in July, Reeves informed Betts that Crystal was only 19 weeks from being ready to fire in U-12e.03a.

While there had been money problems on Vela Uniform through this entire period, apparently the Commission was depending upon a supplemental appropriation to help them out in FY 1962. Vela Uniform had been considered as a two year program to match the political problems, but as far back as January, the ARPA Advisory Committee, headed by Dick Latter, had noted with alarm the intended decrease in money and strongly recommended to ARPA that something be done about it. As the possibility of weapons testing increased toward the latter part of the year, this problem became more serious, especially since ALOO and DMA had started in about June to use some Vela money for weapons test preparation. In early July, DMA (Colonel Anderson) discussed with JCAE the question of parallel efforts on weapons test resumption and the Vela program as planned. The JCAE indicated a strong feeling that the Vela Uniform series be carried out as planned, with foreign observers.

At the end of July, Betts authorized further work on the Linen project, apparently in order to be able to meet the intended shot date the following spring. However, he made it clear, both in July and August, that the shaft should not be backfilled, pending further specific notification, because that would commit the project. By the end of August, H&N estimated that Linen could be ready on May 25, 1962, and that if exploratory drilling of the proposed site were authorized, Lollipop could be ready by January 14, 1963. On August 30, the same day that the Russians announced their decision to resume nuclear testing, Reeves informed DMA of his planning assumptions for Vela Uniform to comply with the funding reduction from $24,000,000 to $10,500,000 for FY 1962. They were: Orchid, 5 kt, tamped in tuff, 10 weeks readiness for a detonation April 15, 1962; Linen, on which work would continue for a detonation date of June 1, 1962; Crystal, on which a 19-week readiness for a detonation on July 15, 1962, would be maintained; and Stingray, where basic construction in U-12e.06 would continue through the fiscal year. As for Lollipop, he would complete the exploration in Area 15 for site suitability and then would need construction authorization. For Shoal, he would conduct an exploratory program. On Porpoise, the five to ten thousand foot 5-kt shot, he assumed no action. On Muslin, he would continue the suspension. As for the off-site shots of project Dribble in the Tatum Dome, he would continue the exploratory program.

With the resumption of testing, Shade, as such, disappeared into history. At the sixth meeting of the ARPA ad hoc group on the detection of nuclear explosions, chaired by Dick Latter, on September 21, 1961, there were the following recommendations:

> The Group reviewed the Vela explosion program in the light of the planned AEC Operation Nougat. The Group concluded that the seismic results obtainable from Operation Nougat should be sufficient to meet the objectives of the proposed Vela Project Shade shots with one possible exception--the deep Porpoise shot. The Group, therefore, recommended that except for Porpoise, Project Shade be discontinued. The need for Porpoise will be reviewed by the Group after seismic data from Operation Nougat are evaluated. The Group recommended that preparations for Project Shoal be continued. The Shoal shot is the only direct comparison of the seimic waves from nuclear explosions with those from earthquakes and, therefore, is vital for an investigation of discrimination techniques. The Group concluded that the original decision that Shoal should be 5-kt nuclear should not be changed.

Shoal was eventually fired, but today (1983) the Linen cavity is still available in

Nevada to anyone who would like to pump it out.

X-Ray Kill Problem, 1961

On March 10, 1961, the Latter brothers (Albert and Richard), Ernest Martinelli, and William McMillan circulated a Rand report they had written that was to cause great controversy and eventually affect the high-altitude program of Dominic appreciably. The report was entitled simply "Some New Considerations Concerning Nuclear Test Ban." (Apparently, Rand did not approve the formal issuance of this report and it is somewhat unclear as to how it got out.) The report made a number of very serious accusations. It started out:

> The United States is seriously considering a nuclear test ban agreement which cannot be adequately controlled. Such an agreement would enable the Soviets to continue their nuclear tests underground and in outer space while the United States would, of course, live up to its commitment. In 1958, at the outset of the test ban talks, it was generally believed that underground nuclear explosions were detectable and nuclear tests in outer space impractical. The facts are now known to be otherwise. In April 1960, hearings before the Joint Congressional Committee on Atomic Energy made it clear that the Soviets can cheat if they want to without fear of the Geneva control system. There is now no essential disagreement among scientific experts on this point irrespective of their views on the desirability of a test ban.

The report brought up the possibility of x-ray kill for incoming nuclear warheads on either side and went into great detail. It envisaged a pack of incoming missiles, including warheads and decoys, on the order of 10 to 15 miles or more in radius, and advised that it might be possible through x-ray damage to the ablation material in the warhead nose cones to achieve a kill radius [redacted] The report suggested that in the event of an inadequately controlled test ban, which would jeopardize the deterrent strength of the United States, it would be necessary to build a deterrent force even larger and more diversified than we would build without a test ban, and, finally, "In our opinion, the best course is to adhere to the principle of adequately controlled agreements. At the present time, this principle allows us to make an agreement stopping atmospheric tests, some space tests, and underground tests above a threshold."

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AFSWC, on April 17, took issue with some of the conclusions of the Rand report, which had the apparent endorsement of John Foster, Edward Teller, and Dave Griggs of the Air Force Scientific Advisory Board. Major Lew Allen* and Lieutenant Paul Hoffman of AFSWC felt that the Rand report had overstated the advantages that the Soviets could gain by clandestine nuclear testing during the uncontrollable test ban, and, in particular, refuted the strong statements made in the original paper that the Soviets could develop an effective antiballistic missile system by secret testing and that the United States could do next to nothing to decrease the present vulnerability of reentry vehicles and complete missile systems. Allen and Hoffman felt that the U.S. could do a great deal to improve present weapons technology in the area of RV vulnerability to nuclear effects, even without nuclear tests, but did comment that:

> The important point is not the testing, but rather that we must continually improve our deterrent capability to survive the Russian defensive capability. In every system which we have examined, the decision regarding possible improvement is not fundamentally affected by lack of nuclear test data at present.

*Lew Allen--Chief of Staff of the Air Force-July 1978 to July 1982.

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[redacted]

At this same meeting, Wigner wondered if Los Alamos would be willing to interest itself in seriously undertaking theoretical calculations on this question, but Betts made the point that the real problem was to simulate experimentally the pertinent weapon flux levels in the Laboratory. The GAC recommended that more effort should be undertaken on the subject now and Pitzer suggested perhaps 25 to 50 bright people full time. Williams commented that it would be difficult to produce experimentally the necessary x-ray flux, short of an actual nuclear explosion. Libby speculated that whether or not one could kill a hydrogen bomb at three miles or so would need to be derived through actual experimentation. Betts agreed to confer with ARPA on the subject.

Apparently reacting to the Rand report, Wigner asked Seaborg to tell Wiesner the GAC would like Panofsky's opinions. Thus, Panofsky, with DASA assistance, assembled a panel to assess the problem for the White House. The Panel membership was Panofsky, Bethe, George Bing, Hendric Bode, Daniel E. Dustin, Richard Garwin, Conrad Longmire, Herbert Scoville, and Spurgeon Keeny. They concluded that U.S. missiles were vulnerable to rather crude AICBM tactics and that, unfortunately, this factor had not been recognized earlier and had not been a missile system design consideration. Whereas the U.S.S.R. was probably thinking of RVs weighing on the order of 10,000 pounds, unfortunately, the U.S. thinking was in the direction of roughly 1,000-pound RVs, which are very difficult to render invulnerable at short distances. The report* suggested that the U.S. should review its reentry vehicles to determine AICBM hardness requirements.

Panofsky, in a discussion with the General Advisory Committee at Los Alamos on July 13-15, 1961, added that, unfortunately, no agency was looking at this problem in its entirety. He stated that there might possibly be short-term remedies for spallation in nose cones, such as changing materials or using foam layers. He also noted that while it was known that the U.S.S.R. was conducting a vigorous AICBM activity, it was unknown whether or not they planned to use nuclear warheads. Panofsky discussed the other possible AICBM kill mechanisms, such as bomb debris, blast, neutrons, beta rays, and non-nuclear pellets. Libby noted that at the moment, the United States was spending roughly $2,300,000 per year on the subject and raised the question of whether or not more should be spent. Panofsky emphatically agreed that more effort was warranted on the problem. Various possible improvements to decrease vulnerability were mentioned, including addition of decoys or protection with weight made available through increase of yield-to-weight ratios, increase of specific impulse of propellants, reduction of missile range requirements, increase of payload capacity, etc. Later in the meeting, Wigner commented that Bradbury and Brown, asked whether they considered the vulnerability problem to be within their province, gave conflicting answers. As a result of this discussion, the General Advisory Committee advised Glenn Seaborg that:

> All currently planned U.S. ICBM warheads and reentry vehicles are vulnerable to AICBM destruction by nuclear explosions at ranges much greater than previously anticipated. This is due not to a single kill mechanism but rather to a combination of several effects. In our opinion the currently planned U.S. family of ICBMs is altogether too vulnerable to crude AICBMs and simple tactics.

*Report of the Ad Hoc Panel on Warhead Vulnerability to the President's Science Advisory Committee, June 20, 1961.

They further commented:

> The recent findings on the vulnerability of our ICBM and the unexplored possibilities of AICBM initiated by the March 10 Rand report point not only to a present military danger, they also raise the question of whether our weapon research has been set up in the way which ensures that no catastrophic surprises are in store for us. If one compares the present situation with that which existed during the Manhattan project, one cannot but deplore the absence of a group of scientists feeling a true responsibility for all aspects of weapons research and its consequences and who devote all their time to these problems. Unless we succeed in establishing such a group of first-rate scientists with an intense and abiding interest in weapons research and the military strength of this country, surprises similar to the present one will recur. Our concern applies not only to nuclear weapons, but to all weapons and to their integration in systems. The recent events show that the present setup is unsatisfactory in view of the fact that even a temporary clear military superiority of our opponents may have permanent effects. We recommend that the AEC should have its weapons laboratories assume the broader responsibility of examination of the entire weapons systems in which its warheads are employed.

This period marked the beginning of the transition from the philosophy of neutron kill of incoming missiles to x-ray kill. Obviously, many variations of this were discussed over the coming years.

Vela Sierra, January-August 1961

The Vela Sierra equipment destined for Thule, Greenland, was received at Los Alamos from EG&G during January, was further prepared, and then shipped to Greenland. Prototype fluorescence detection stations operated during February at Fairbanks (Operation Big Moon), and at Thule (Operation Brass Ring). A partial prototype of the direct optical system, prepared by EG&G for LASL test, was received at LASL during February.

The main objectives of Operations Big Moon and Brass Ring were to study natural backgrounds and to ascertain the capabilities of the air fluorescence system in the auroral and polar cap latitudes, and more generally, to carry out an operational evaluation of the "preproduction prototype" air fluorescence equipment constructed by EG&G for LASL. Although the data were not fully reduced by late March, indications were that the aurora did not give optical pulses which would lead to false alarm signals. The prototype check out was considered successful, and the solution or elimination of a rather long list of problems and malfunctions was now possible. Moreover, these experiences made it possible for the Air Force to build an operational station. LASL planned to collect data on the natural background, and then to derive the appropriate conclusions regarding the operational capability of the Geneva-type international system.

By the end of July the set of air fluorescence detection equipment that had been returned from Thule was operational in a room atop the administration building at LASL, largely through the efforts of R. Thompson, an AFTAC noncommissioned officer who had assisted at Thule. Following the completion of modifications being effected by EG&G, another set of instrumentation was utilized for routine observations of natural background during the remainder of the summer. Many serious electronic troubles had turned up in the direct optical system, and were gradually being eliminated.

During August problems of calibration, increase in range, signal recognition, and auroral background were addressed using the prototype systems. LASL and EG&G discussed the need to record world time as an aid to detection, and all concerned agreed that the present design was hopeless for this application. It thus appeared likely that an entirely new version would be required to replace the prototype, but no decision had been reached by mid-August.

Vela Hotel, January-August 1961

LASL continued instrumentation flight tests in early 1961. Proton telescopes, electron scintillation spectrometers, ionization chambers, and other equipment were tested on Atlas and Blue Scout flights.

ARPA published a new Vela Hotel order on January 13, naming a joint technical group to be chaired by ARDC, with representation from AEC and DOD, which would have technical supervisory authority over AEC and DOD portions of the program. Areas of responsibility were defined as follows: AEC would provide detectors, logic systems, and analysis of telemetry; ARDC would provide satellite vehicles, including integration of detectors and logics systems, system assembly and testing, and other items. Thus, the AEC desire to have responsibility for the complete satellite, including the interface with the booster, was not realized.

The conclusions and recommendations of the ARPA ad hoc group on detection of nuclear detonations, chaired by Dr. Richard Latter, were forwarded to General Betts (Director of ARPA) on January 16. Without noting the specifics, the group stated that "the funds available for Vela Hotel are still far below those required for a scientifically desirable program. The group urges that the required funds be released as soon as possible to this program."

The Vela Hotel Joint Planning Team met on February 14 and 15 and recommended that the original March 30, 1960, development plan be followed at a cost of approximately $100,000,000, but they also offered, as an alternative, a detailed discussion of a limited program consistent with current funding limits. This limited program would lead to three launches using the Thor/Able Star/30KS 8,000 booster system launched from the Atlantic Missile Range. LASL/Sandia would have 90 pounds in which to provide a useful payload. The orbit would have an apogee of 50,000 nautical miles and a perigee no less than 30,000 nautical miles, with a 50,000-mile circular orbit preferred. The AEC was asked to have by March 1 a draft payload description for use in making a new development and funding plan to be finalized by March 16. This updated plan, published on March 9, entitled "The Vela Hotel Program, Joint Development Plan (Reduced Scope Plan), ARDC/AEC/NASA" noted that it was in response to ARPA Order No. 102-61, Amendment No. 3, dated December 8, 1960, which provided guidance for a reduced scope program of about $10,000,000. It was proposed that the new program consist of two phases. The first phase would be a group of piggyback and probe experiments, and Phase II would place at least one of three spacecraft into a successful orbit by about 18 months after approval. The program's goal was to gather sufficient satellite data on the space radiation backgrounds to fully define an operational detection system. Based on a program approval from ARPA by April 1961, the Thor carrier system was to be developed to support the launch of satellites to be delivered in May, July, and September of 1962, with launches scheduled for October and December 1962 and February 1963. By mid-March the program had been expanded to the $25,000,000 level.

Dick Taschek reported the status of the instrument development flights to LASL Director Norris Bradbury on April 3, summarizing the program of some 25 flights (on various Atlas launches, 609A rocket probes, Blue Scout Jr. probes, and upcoming NASA Moon and Venus probes) as having had a high degree of success. Flights to the region beyond the radiation belts had not yet been achieved. Planning was now under way for the next series of detector flights in Nike-Cajun rockets. But, during April, the first two attempts to launch balloons in the BLICOS program failed at the Tonopah Test Range in Nevada.

By May the decision had been made to use a launch system (Atlas-Agena) with a much greater lift capability than the Thor system. Although the first flight was now planned not to be before March 1963, each launch could carry two spacecraft plus additional weight. A meeting of the joint planning group in the near future would address the use of the higher weight capability. The new overall program might involve as many as 12 satellites. The overall DOD funding plan now was $64 million, to be spent through FY 1965. On May 24 General Betts (now Director, DMA) asked Hertford of ALOO, who handled the overall Laboratory funding for Vela Hotel, to review the developmental requirements in accordance with the new plan and submit the AEC requirements. The response from Hertford on May 31 included increased funding requests for Sandia and LASL for FY 1962 and the three years following. For FY 1962, about $3 million total, instead of $2 million as earlier authorized, would be required. ARPA issued an order to Air Force Space Systems Division (SSD, formerly part of AFBMD) during June giving formal approval for the Atlas-Agena concept.

The details of the new Vela Hotel concept were made more definite in a development and funding plan updated in July 1961. The specific goal of the program was still to gather sufficient knowledge of the space radiation background to permit an operational detection system to be fully defined. However, the increased payload capability now allowed each launch to inject (using separate kick rockets) two spacecraft into different 50,000 nautical mile circular orbits. The extra payload weight capability had been used up mostly by the new rocket motors, with a little weight being used by additional payload systems. Each spacecraft would now carry x-ray, gamma ray, and neutron detectors, as well as the equipment to accumulate and store data which were to be transmitted later on ground command. Five launches were planned at three-month intervals beginning in April of 1963.

LASL froze the design of their part of the spacecraft in August, noting that within a few weeks initial negotiations would begin with the spacecraft contractor, to be selected in September. Both LASL and Sandia sent a representative to SSD in Los Angeles for a two-week period to serve as technical advisors in the selection of the contractor.

Plowshare, January-August 1961

The Vela Advisory Group meeting on January 5 and 6, 1961, briefly addressed the mutual usefulness of Vela Uniform and Plowshare detonations. They concluded that the Plowshare Gnome detonation scheduled for fall 1961 was of direct relevance to Vela Uniform and recommended that it be instrumented to the maximum extent feasible without interfering with the overall Vela program. Furthermore, they recommended that Vela Uniform detonations be made available for Plowshare experiments, provided such experiments didn't interfere in a major way with Vela.

A new Plowshare project, known as Wagon, was explained in a document entitled "Technical Director's Concept of Project Wagon (Danny Boy)," first published on January 14, 1961, and updated in March. The objectives of the test would be to learn more about the cratering capabilities of buried nuclear explosives, the characteristics of seismic shock and airblast, and the amount and distribution of radioactivity, among other things. The tentative plan was to detonate a 1-kt nuclear device in basalt 200 feet below the surface at the NTS. The area most nearly satisfying the criteria was the Buckboard site in Area 18, where Sandia had carried out high-explosive cratering tests the previous summer. Clifford M. Bacigalupi of Livermore would be the technical director. A flexible schedule was laid out which would allow the detonation to take place about six months after authorization to proceed.

The Livermore mid-year program letter of January 18 from Harold Brown noted the detrimental effect on the Plowshare Program of the political considerations that forbade nuclear detonations. However, it was also noted that Vela experiments such as Cowboy were contributing data for Plowshare. At this time, Gnome, the only firmly projected nuclear Plowshare test, was planned for August of 1961, only seven months away.

On February 2, 1961, John J. McCloy sent the Commission a paper covering the issues of the nuclear test ban negotiations and requesting their considerations and comment. He recommended that the U.S. now indicate that it was prepared to drop the requirement that Plowshare tests be performed from a stockpile of devices placed under international surveillance before the treaty entered into force, but wherein the internals of the devices would not be subject to inspection. The requirement was to be replaced with a proposal for "disclosure of devices and their blueprints by the testing country and an agreed upper limit on the number of shots by a single party in an agreed period of time."

Some indication of future Plowshare activity was given by the Commission's FY 1962 budget discussions in the meeting of February 8, 1961. A proposed increase of $7.9 million would provide $4.9 million for site preparation and construction for Project Chariot in Alaska to meet a shot schedule in the spring of 1962, $3 million for site preparation and construction for developing and field testing nuclear explosives (Project Ditchdigger) in Nevada, and initial site preparation for an experiment intended to study very high pressure effects on chemical reactions. While it was emphasized that funding was dependent upon receiving Presidential approval for resuming nuclear detonations, the Department of State had expressed interest at a National Security Council meeting on January 18 in developing the canal construction possibilities of Plowshare. While this lent some impetus to carrying out Project Chariot, Commissioner Graham hesitated to move forward with Chariot without confirming its desirability with the Administration. Mr. Kelly of the Plowshare Advisory Committee noted that the Chariot tests could be carried out only in the spring because of weather and biological conditions, and if, after preparations, it were delayed from its spring of 1962 firing date, it should be held in readiness for later use. Commissioner Wilson suggested that the funds be included in the FY 1962 budget amendment, with a decision to be made later about the desirability of a 1962 firing date. Mr. Kelly, noting that the firing date for Gnome was not to be set until after site preparation, suggested that a similar path could be utilized for Chariot. Further discussion among the three Commission members (Wilson, Olson, and Graham, the acting chairman), which included the questions of safety and radioactive contamination, led to a decision to defer consideration of this budget request.

Two days later the Commissioners and Seaborg, Chairman-designate, met with the Plowshare Advisory Committee. Spofford English, chairman of the Committee, began with a discussion of the Ditchdigger project which the Committee strongly favored. Aside from its most obvious usefulness as an excavation tool, it would also produce a large amount of heavy nuclides such as californium. The development of this type of device was absolutely necessary, said English, to minimize problems of radioactive contamination from large-scale detonations and, moreover, could yield information in unknown areas of basic chemical and nuclear reactions. Further discussion of Ditchdigger addressed the problems of designating such tests as weapons development; research in chemical reactions under extreme pressures; and that information on fission fractionation and radioactive contamination which could be obtained from developmental experiments. The Committee recommended unanimously that Chariot should proceed to a spring 1962 test, expressing confidence that it could be conducted safely. This confidence was based on results from the Chariot bioenvironmental survey program and on estimates of the amount of radioactive products which would be released. The AEC General Manager, Alvin Luedecke, expressed the opinion that it would not be wise to commit FY 1961 funds until it was indicated that the program could proceed in FY 1962. In regard to the overall Plowshare program, Mr. Abelson of the Plowshare Advisory Committee reiterated the Committee conclusion that, in the context of the AEC's total research effort, Plowshare held the greatest promise for dramatic breakthrough in areas now totally unknown. Mr. English stated that the Committee wished to see additional funds provided for measurements on Vela Uniform tests that would be useful to the Plowshare program and that would provide data not obtainable otherwise. He estimated that the required funding increment was approximately twice the budget for high-explosive experiments. Mr. Seaborg, however, noted that plans to do additional measurements on the Vela shots might lead to the suggestion that there was a U.S. effort to circumvent the weapons test moratorium, thus overshadowing the primary purpose of the seismic tests. Discussion of Project Gnome, scheduled for November 1961, addressed the possibility that the explosion would create surface fissures which would allow venting of radioactive material. To minimize this problem, the depth of burial could be increased (from 1,200 feet) or the yield could be decreased. It was decided that in order to stay within the budget, the yield should be reduced from 10 to 5 kt and it was agreed that a reduction would not appreciably affect the anticipated results.

In response to a State Department proposal that the U.S. unilaterally disclose Vela device designs to the Russians, the AEC staff pointed out to Mr. Adrian Fisher, McCloy's deputy, their reluctance to agree to that approach because of its possible adverse effect on the Plowshare program, an effect which could arise because the Plowshare devices might involve weapon concepts and device improvements (although specifically for Plowshare) that we would not wish to disclose to the Russians. Commissioner Wilson felt that revealing the Ditchdigger device design to the Russians would be a significant revelation of advanced design techniques to which he was opposed. He felt that Plowshare would fail if, in order to keep those designs from the Russians, the required tests were not conducted. He stressed that he could not agree to giving up the advantages of Plowshare in order to gain an illusory test cessation agreement with the U.S.S.R. Commissioner Graham, on the other hand, feeling that the Commission's past reluctance to alter its position on design disclosure had weakened its position in the public eyes, expressed concern over the Commission position that Plowshare was for peaceful purposes, but information and designs were being withheld for national defense reasons. He further suggested that the Commission consider open demonstrations. After more discussion, and in spite of the opinion of one or two of the Commissioners, the AEC staff remained convinced that, whereas revealing device design and allowing internal inspection was acceptable for seismic research (in particular, the Mark XI), that procedure was not acceptable for the Plowshare devices. Discussion seemed to indicate this position would not change until Congressional attitude on the subject was clear.

On February 16 the new President, John Kennedy, met with the AEC Commissioners at Germantown. During the meeting the subject of Plowshare came up, and the minutes of that meeting include the following:

> The Plowshare program for the use of nuclear detonations for peaceful purposes was a program of great interest to the Commission, Mr. Seaborg said, and one which at the moment is making little progress. Mr. Wiesner remarked it is clear that the use of nuclear detonations for peaceful purposes offered great possibilities and test cessation should not preclude the many benefits such a program could bring to the world. The problems here are, of course, how to use nuclear devices for these purposes and still disprove any accusations that they were being used for weapons development purposes, a difficulty posed by the fact that advanced weapons, which cannot be shown, will be used. He thought this subject was a matter of such importance that it would be useful to set up a (special) briefing for the President on the Plowshare program.

Nevertheless, when the Geneva talks resumed on March 21, Ambassador Dean announced that the U.S. was now willing to accept the same safeguards for both Plowshare and seismic explosions, meaning that U.S. nuclear devices to be used would be open to Soviet inspection and that we would require the same of any devices they intended to use for nuclear experiments. Dean acknowledged to the conference that this would limit the U.S. Plowshare program to use of obsolete devices. He outlined some of the objectives of the Plowshare program as follows:

a. Developing techniques for major earth-moving enterprises, such as the construction of harbors or canals;

b. Using peaceful detonations to break up rock, regulate the underground flow of water, and to make it possible to recover petroleum from shale deposits in the United States and tar sands in Canada; and

c. Exploring the possibility of producing heat and power economically and of producing isotopes and chemical reactions for use in many peaceful pursuits.

On the other hand, he said, we did not agree to the one-for-one stipulation of the Russians' February 1959 counterproposal on peaceful detonations, requesting them to withdraw this demand so that either side could carry out a Plowshare-type program and not suffer a veto because the other side simply refused to do any tests of their own.

On May 17, 1961, Livermore published the "Technical Director's Operations Plan, Continuing Chemical Explosive Experiment at NTS," also known as Project Rowboat. In this project, a series of multiple, simultaneous, high-explosive detonations, were to be carried out in June 1961 in Area 10 at the NTS, on the western edge of the Stagecoach area. Area 10 was chosen because of the similarity in geology to sites which had been used earlier for gathering data on seismic signals and cratering characteristics. The project, carried out under the technical direction of Livermore (Clifford Bacigalupi), included eight different tests, each consisting of several (three to six) simultaneous high-explosive detonations buried from 12 to 18 feet deep.

On May 31 the Commission discussed revisions to the Plowshare program resulting from the new Geneva policy on safeguards and device design disclosure. General Betts noted that there was no obsolete device of the proper yield for the Chariot experiment. When it was proposed that the feasibility of redesigning the experiment be investigated, Commissioner Wilson suggested that this be delayed for a month or two in case the Geneva talks reached a point or a conclusion that would make such a redesign unnecessary. After discussions covering public information policy, budget distribution, and program revision, the Commission approved a revised program which included implementation of a broader and more informative public information policy; continuation of R&D at a slightly reduced level; and reorientation of the projects so as to prepare Gnome for firing in FY 62, to defer further work on Ditchdigger and Limestone, and to support, at reduced levels, the Chariot bioenvironmental survey work and the Wagon preparations.

The next meeting of the Plowshare Advisory Committee was held at Los Alamos on June 7 and 8. The committee was not surprised that the current budget requests were still not being fulfilled, but felt that several factors, especially the international situation, made it prudent to have substantial plans on hand for specific projects which could be prosecuted rapidly "as soon as the situation is clarified." Moreover, the Committee pointed out that substantial funds would be needed immediately if the international situation were to allow pursuit of the Plowshare projects. Specifically, if further device development became possible, the Committee would recommend that such work receive first priority and funds be provided immediately to pursue this avenue so as to attain the least possible radioactive contamination from detonations. Noting the unfortunate fact that much of what had been accomplished was classified, they suggested the classification rules be modified to permit some public understanding of these device possibilities. In addition to the clean device concept for Ditchdigger they also encouraged research directed to this end by other means. They expressed their desire to make use of these developments on Project Chariot.

Other items in Chariot discussions included the preliminary conclusion by Livermore that a scaled-down experiment which could be done with a "disclosable" device was less desirable than waiting for Chariot to be done properly. The special subcommittee on Project Chariot made their interim report and concluded that more information, analyses, and studies were required regarding the bioenvironmental and safety aspects of the project. Among the other subjects discussed by the Committee were Project Gnome (the Committee urged that the experiment be carried out on schedule), Project Wagon, nuclear explosions for various kinds of scientific research, and the AEC's recently adopted public information policy (which the Committee endorsed and urged be implemented immediately).

In June and July 1961 there was discussion of alternatives to continuing the moratorium. An announcement of U.S. test resumption underground could be made in various ways. The Geneva delegation warned that any ban on atmospheric testing must be carefully worded to protect our intent and potential needs for Plowshare cratering shots.

The AEC's preparations for the Gnome event near Carlsbad, New Mexico, became publicly known in July when a couple of newspaper articles and exchanges with the AEC brought out the fact that this test, "for purely peaceful scientific research," had progressed to the stage where it could, if authority were given, be performed in December 1961. Project Gnome, which was actually carried out on December 10, after test resumption, will be discussed in greater detail in Chapter III.

Additional bioenvironmental studies for Project Chariot had been carried out during 1961, but the earliest date being considered for that test was the spring of 1963. A project named Coach was also being planned at Livermore. Coach, using a several kiloton nuclear explosive especially designed to produce a high neutron flux, was intended to study the possibility of the production of neutron-rich isotopes of known trans-plutonic elements and of elements heavier than those yet discovered. Re-use of the Gnome site was being considered for Coach.

In an effort to gather Plowshare-related data from nuclear tests performed in the past, soils (and where possible, plants and animals) from the environs of Teapot Ess in 1955, Buster-Jangle in 1951, Rainier in 1957, and Blanca and Logan in 1958 were studied.

Plowshare-type activities were put under a new division in AEC headquarters--the Division of Peaceful Nuclear Explosives (DPNE)--during 1961.

Deep Space, January-August 1961

DASA's request for authorization to proceed with the Advanced System for Weapons Test (ASWT) was stalled by Herbert York (DDR&E) on February 22, 1961, pending further policy guidance expected within the next six weeks. York also deferred a decision on whether the operation should be planned for a remote site or the Atlantic Missile Range, noting that the Assistant Director for Ranges and Space Group Support had indicated no overriding technical reasons for not performing the ASWT tests from AMR. However, York noted that the decision would ultimately have to be made at a national level.

At the meeting of the AEC General Advisory Committee toward the end of April it was recommended that plans be made for exoatmospheric tests. In the subsequent AEC discussions on a capability for such testing, Betts reminded Luedecke that the AEC had participated with the Air Force in a joint feasibility study and proposal, published on August 29, 1960, (Advanced System for Weapons Test) which described a plan for outer space testing with a lead time of 18 to 24 months, and that he didn't plan, for the time being, to initiate any more studies.

A May 12 letter from Sandia to General Betts forwarded a Sandia report, No. SC-4575 (WD), entitled, "A System for Weapons Development Tests in Space."

On June 22 the Chief of DASA sent to the Joint Chiefs of Staff a "review of nuclear test plans" which had a number of sections on DOD needs for various types of weapons effects tests in various environments. That review showed that two years would be required to develop a space testing capability and perhaps additional time would be needed to develop instrumentation for obtaining worthwhile effects data.

In providing the details of outer space test readiness to the Commission on June 27, General Betts gave a brief background of the earlier studies for outer space test capability which led to the ASWT report in 1960. He stated that the first test would be for calibration, and would use [redacted] This would be [redacted] The first tests would utilize diagnostic capsules and measure yield (by x-rays) and time interval. The cost for a five-launch program would be about $40 million if a U.S. launch site were used or about $100 million if overseas launch facilities had to be built. The latter method would require about two years before first launch, whereas utilizing an existing U.S. launch site might allow a test within about 18 months.

Withheld Under 5 U.S.C. 552 (b)(3) DOE, Exemption 3

On June 30 DASA sent a copy of the ASWT study to the DOD Office of International Security Affairs to be forwarded to the Disarmament Administration (USDA) for study.

Following their meeting at Los Alamos on July 13 through 15, 1961, the General Advisory Committee summarized their comments and recommendations in a July 19 letter to Chairman Seaborg. Feeling that "preparations for resumption of nuclear testing are seriously inadequate," the Committee recommended various activities to increase AEC readiness, including preparation of plans and equipment for outer space nuclear tests on an urgent time scale. "Nonnuclear tests of such facilities could be undertaken even during the moratorium."

On August 7 Colonel Anderson of the DMA test office sent a memo to Chairman Seaborg which included estimated costs per test for various types of test methods. For the outer space method it was estimated that with the ASWT system the first test would cost about $100 million and each shot thereafter would be $10 to $15 million.

On the same day General Betts sent a message to Los Alamos and Livermore asking them to look at the use of outer space testing for various weapons test requirements and, assuming that underground testing would also be permitted, to address the advantages and disadvantages of the two methods. In the replies from Foster on August 24 and Bradbury on August 30, both stressed the high cost and tremendous effort involved in developing such a capability. The safety problems to be overcome were emphasized and the advantages (high-yield testing and measurement of certain types of effects which were not possible underground) were also stated. However, the flavor was certainly that neither laboratory would recommend developing the capability at that time, but Bradbury did point to the engineering studies that had been done in the past by Sandia on such a method. In discussions within LASL, Harold Agnew told the Weapons Working Group on August 8 that J-Division had advised Sandia to base their outer space testing proposal [redacted] In discussions within J-Division, Ogle suggested to Graves that the large potential safety problems with exo-atmospheric testing were such that deep space test planning (with the exception of the delivery system) should not be pursued by LASL until the proper effort could be put into it.

Withheld Under 5 U.S.C. 552(b)(3) DOE Exemption 3

Domestic and International Political Developments, January-August 1961

The year 1961 opened with numerous changes in the key personnel filling high-level government positions. Ambassador Arthur H. Dean, Kennedy's newly appointed chief of the U.S. delegation at the Geneva test ban talks, later recalled in his book, *Test Ban and Disarmament*, that the test ban talks were in recess while the status quo was being fully analyzed in the the early days of the Kennedy administration (which began on January 20, 1961) and a comprehensive "new" approach at Geneva was being formulated.

> By the end of 1960 it was hard to know how to draw the balance (of basis for hope or despair in the test ban talks). On the other hand, agreement had been reached on a preamble, 17 articles and 2 annexes of a draft treaty for a comprehensive test ban, including 1 article which recognised the principle of international inspection. On the other hand, the diplomatic atmosphere had deteriorated markedly since the collapse of the summit conference in May 1960 after the U-2 incident.

President Kennedy solicited the full spectrum of scientific and political opinions and "ultimately decided, in part on the basis of the report in February-March 1961 of a special committee under Dr. Fisk, and after a favorable recommendation by the National Security Council, that it would be to our national advantage to work for a comprehensive test ban treaty."* Kennedy, in his first State of the Union message on January 29, requested a "reasonable delay" in the Geneva talks, stating that we intended "to resume negotiations prepared to reach a final agreement." The re-thinking of our Geneva position and hard looks at the potential weapons status and development potential for both the U.S. and U.S.S.R., in or out of a test moratorium, were evident early in the administration.

In late January, the "disarmament study group," chaired by Dr. James Fisk and reporting to Mr. John J. McCloy, the President's new disarmament advisor, undertook a study on the potential weapon developments possible in both the U.S. and U.S.S.R. under various hypothetical testing scenarios. The context of this study was intended to update the "McRae Report on Weapons Testing."

Withheld under 5 U.S.C. 552 (b)(1) 1.3(a)(4) DOE EXEMPTION 1

During late January and February a series of discussions on the Soviet weapon program, facilities, and test capabilities took place between the weapons laboratories, [redacted] and the AEC. Starbird noted at the January 28 Commission meeting that the Fisk panel was addressing five major problem areas as follows:

a. The capabilities of the present Geneva system to monitor a nuclear test agreement.
b. The estimated capabilities of a revised Geneva System, after extensive research and development, including Project Vela.
c. The probable gains to the United States from conducting various types of nuclear tests.
d. The probable gains to the U.S.S.R. from conducting various types of nuclear tests.
e. A comparison of the relative gains to each side in terms of improved weapons systems.

The Commission was anxious to be informed of deliberations of subgroups in which Starbird, Carson Mark, Harold Brown and others were participating, in order to be prepared for upcoming discussions with the Joint Committee on Atomic Energy. However, reports of those deliberations indicated vastly differing opinions on the various questions with no predictions of what conclusions would result.

In early February 1961 McCloy informed the commission of the major issues still open at Geneva and presented strawman recommendations as to what positions the U.S. might take on such issues as safeguards for seismic research and Plowshare detonations, composition of the control commission, and on-site inspection quotas for their

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*Arthur H. Dean, *Test Ban and Disarmament: The Path of Negotiation*, New York and London: Harper & Row, 1966, pages 6, 87, 88.

use in formulating instructions to the Geneva delegation before resumption of talks on March 21. At the March 1 Commission meeting General Betts reviewed that part of the Fisk panel's report dealing with the capabilities of the AFTAC detection system, the capabilities of the Geneva system proposed by the U.S., estimated improvements of the Geneva system, the present status of nuclear weapons technology, potential future weapons developments in the U.S., U.K. and U.S.S.R., and the cost necessary to evade the proposed Geneva system.

> General Betts said members of the panel generally agreed that underground and high-altitude testing could be conducted without detection if adequate steps were taken to conceal it. He stated the cost would vary with the steps taken to avoid detection of underground tests. He stated there was little agreement among the members of the panel on the cost of the clandestine high-altitude testing, even though the panel noted the U.S.S.R. possessed rocket systems with a high thrust potential suitable for testing at high altitudes in the megaton range.

There was wide disagreement over what further weapons development might be possible by various types of testing.

Dr. Jerome Wiesner, the President's new science advisor, had participated in former Commissioner Murray's campaign exchange with the Presidential candidates the previous fall. In earlier newspaper articles he had criticized the technical preparation by the American delegations to various conferences, including the test ban talks, and opined that the U.S. "has generaly been ultraconservative in the inspection requirements it places upon any system." He seemed to feel that inspection systems of a wide variety would be possible and that a monitoring system could be developed that would be deemed adequate to monitor any degree of disarmament, "though its acceptability is by no means certain." He had also been recently quoted as stating, "I know of no reason for resuming testing immediately. My own view is that the U.S. cannot let any single thing hinder the negotiations." Thus, his statements and positions gave the overall feeling that the U.S. had not been earnest in exploring any and all avenues toward arms control, disarmament, inspection, and related issues.

After three months of behind-the-scenes discussion, Ambassador Dean opened the Geneva talks by stating a U.S. position which was new on several items. Among other things the U.S. offered to reduce the number of detection control posts in each country (e.g., from 21 to 19 in Russia), to accept an 11-state treaty control commission with East-West parity (four western, four Soviet bloc, and three neutrals), to allow Soviet inspection (with Congressional/AEC concurrence) of U.S. nuclear devices to be used in the Vela Uniform and Plowshare programs, and to agree on a total high-altitude test ban and the necessary technical equipment for control.

Ambassador Tsarapkin of the Soviet Union, in turn, stated a new U.S.S.R. position by withdrawing agreement for a single administrator (of the control commission) and proposing a three-man executive (one Soviet, one West, and one neutral). This plan, to become known as the Troika, coupled with the Soviet demands for unanimity on major control commission action, was tántamount to a Soviet veto on such things as on-site inspections. Tsarapkin also raised the question of French nuclear tests for the first time in the Geneva negotiations, accusing the U.S. and U.K. of prolonging the talks in order to give their NATO ally time to conduct these tests, and implying that France might be testing for the U.S. and U.K.

The impact of the exchange in Geneva after the long recess was expressed in Ambassador Dean's own words, who said that the Soviets had:

> . . . set back our hopes by introducing . . . a "Troika" proposal that would have stultified the operation c. the proposed international control organ. Since a similar arrangement had already been discussed by the U.S. and U.K. in 1958 and rejected, and since even the Soviet Union had assented to an alternative approach, the Soviet reversion to an abandoned position did not make for immediate optimism.

Dean labeled the "Troika" proposal as unacceptable on March 30 in Geneva, and President Kennedy publicly stated his discouragement over this Soviet proposal in a news conference on April 21. Expectations of rapid action toward a treaty under the new administration were short-lived because of the harder, uncompromising Soviet position.

Nevertheless, the U.S. and the U.K. tabled a new "comprehensive" treaty on April 18 calling for the following conditions:

a. Ban on tests everywhere except underground tests producing seismic signals less than magnitude 4.75.
b. Voluntary three-year moratorium (renewable annually thereafter) on underground tests below magnitude 4.75 pending perfection of techniques for detecting small underground tests.
c. Up to 20 on-site inspections annually in the territory of each of the three powers. Inspection teams would not include nationals of the country inspected, except as observers.
d. Direction of the treaty control commission by a single administrator acceptable to all three powers.

The next day Ambassador Tsarapkin rejected the Western treaty proposal, reiterating the Soviet "politically determined" limit of three on-site inspections annually and the demand for Russian representation on any team inspecting the U.S.S.R. The Russians cited the recent actions of U.N. Secretary-General Dag Hammarskjold in the Congo as the basis for their demand of a Troika administrative council rather than a single administrator.

While the above mentioned action was going on in Geneva, back home the U.S. Disarmament Administration set up the "Consultative Group on Nuclear Armaments" chaired by Harvey Brooks of the USDA, to discuss steps that could be taken toward a disarmament agreement with Russia, a subject that was related to the test ban talks. Members included, among others, Norris Bradbury and John Foster. A letter from Bradbury to Brooks on April 28 contains these interesting comments:

> It is possible that the intransigence of the U.S.S.R. in the test ban negotiations is due to the fact that they do not regard the game as worth the candle. It is perfectly obvious that the current difficulties over inspection procedures, vetoes, and so on would, if extended into a disarmament situation, make it completely unworkable. Some real steps toward disarmament (e.g., stopping production of fissionable material for weapons) might seem to them worth more real effort.

Bradbury felt that such steps would not create an unacceptable national security risk.

The Brooks panel had asked a group known as the Perkins panel to work out the details of the U.S. position along with formulation of the advantages and disadvantages of various positions. The panel did agree on certain recommendations, e.g., cutting off the production of fissionable materials for weapons use, but it warned against tying any of the conditions too closely to the existing unmonitored test ban. Specifically,

> The U.S. should push vigorously for a resolution of the present test ban negotiations in order to retain its freedom of action in regard to testing in the absence of a satisfactory agreement on an adequate control system . . . the West should be prepared to exploit fully the political initiative it has acquired in world opinion through the change in the Soviet position on the control administration. Two of the key issues unresolved . . .are an adequate control system . . .and an impartial administration for the international control organization. A solution to these issues must be in sight before there can be hope for meaningful progress in any of the disarmament measures known to us.... The U.S. should be prepared to initiate unilateral underground nuclear tests for purposes of seismic improvement on short notice . . . the U.S. should be prepared to initiate a well-planned series of underground weapons tests . . . the panel urges a reexamination of our present preparations for resumption of testing . . .

After the Soviet's words in March about French testing, the French performed their fourth (and last for some time) atmospheric test in the Sahara on April 25. Ambassador Tsarapkin then stated that further French testing would make agreement on test cessation impossible and that continuance of French tests "places the Soviet Union in a situation which may compel it to resume" nuclear tests.

Back in Geneva April 28 was a milestone in that it was the 300th meeting since the beginning in late 1958, and the statements of the two sides were particularly telling. Dean was quite pessimistic and talked of a future of innumerable meetings, saying: "To me it seems much more likely that within some reasonable period, our fate will have been determined and our success or failure written down upon the pages of history." Tsarapkin responded, stating that the U.S. and U.K. statements had given "the death knell of our conference." He made it clear that the Soviets were not going to back down from their "Troika" proposal and claimed that the West wanted to wreck the negotiations and shift the blame to the other side. Several days later, on May 5, Kennedy made another public statement citing the "Troika" proposal as unacceptable.

On May 16 John McCloy sent to Chairman Seaborg a communication entitled "Memorandum on Future United States Policy Regarding Negotiations With the Soviets on the Test Ban." McCloy's paper, which he proposed be discussed at the meeting of the Principals on May 22, included:

> It now appears clear that the intent of the Soviet negotiators at Geneva is to avoid a prompt conclusion on test ban agreement. . . . According to Ambassador Dean, the indications are that the Soviet Union will not take the responsibility of breaking off negotiations, but will drag them along to a time when they might be merged into the comprehensive disarmament negotiations now set for July 31, 1961. Ambassador Dean's estimate is that the Soviets are trying to put the U.S. in a position in which it goes into the July 31 talks with the present uncontrolled moratorium still in effect, a position which would make it increasingly difficult for the United States to exercise its freedom of action with respect to the resumption of testing. This difficulty might be intensified by the agreement to begin discussion with the U.S.S.R. sometime in mid-June concerning the conduct of these negotiations.

After going into possible Soviet motivations for their position and suggesting ways to resolve some of the problems, the paper continued:

> . . .decision should be made now so that a course of action can be planned before the scheduled resumption of the comprehensive disarmament negotiations on July 31, 1961, and the beginning of discussions with the U.S.S.R. in mid-June. Two questions must be decided: Should the U.S., sometime in June or July of 1961, indicate that it is preparing to resume nuclear tests? Should the U.S. actually detonate a nuclear device prior to July 31, 1961, and if so, what sort of nuclear device should be detonated?

Further discussion addressed the type of detonation and the arguments for and against test resumption.

On May 29 the U.S. and U.K. did make a significant compromise by changing their position on annual inspection quotas from 20 down to 12, that quota to be achieved at a ratio of one inspection for every five eligible seismic events. The Russians rejected this "sliding scale" proposal on May 31, saying that an acceptable number would have to be determined politically, not technically.

The Vienna summit talks between Kennedy and Khrushchev, requested by Kennedy in February, took place on June 3 and 4. One of the topics of discussion was the almost hopeless test ban question. The results matched the expectations. Kennedy told Khrushchev that the U.S. Senate would never approve a test ban treaty with a Soviet veto such as the Troika provided. Khrushchev answered that the Soviets would only drop the Troika proposal if the test ban matter would now be included under talks for complete disarmament. Khrushchev had argued in Geneva that the Soviets couldn't accept controls which they considered equivalent to espionage. "Kennedy suggested that if the controls turned out really to threaten Soviet security, the Soviet Union retained the right to abrogate the treaty." Kennedy also informed Khrushchev that combining the test ban with disarmament discussions would cause the uninspected moratorium to continue for several more years and noted that the American people were already concerned over the protracted uninspected moratorium on testing. Kennedy's public statement after the summit, on June 6, was that hopes for an end to nuclear testing had "been struck a serious blow."

The Soviets made their Vienna memorandum more firm on June 12 by presenting the conditions at Geneva. In essence they issued an ultimatum to the West that either the Troika and three inspection quota proposals be accepted or the test ban issue be merged with general disarmament discussions. Ambassador Dean immediately rejected the Soviet proposals as an attempt to "dictate" to the conference.

The U.S. formally warned the Russians on June 17, in an aide-memoire, that the security of the free world did not permit an indefinite continuation of the U.S. test suspension "without the certainty that the Soviet Union had likewise stopped its testing." The U.S. said that combining test ban with disarmament talks was unacceptable and called on the Soviets to reach, with the West, an effective test ban without delay.

On June 20 Ambassador Dean was recalled to Washington and replaced by his deputy, Charles Stelle, as an expression of the U.S. belief that the talks were hopelessly deadlocked.

The Soviets answered the June 17 U.S. aide-memoire in early July, again stating that a way out of the deadlock should be sought by joining this issue with complete disarmament. The Americans formally replied on July 15 with no change in their positions, almost pleading with the Soviets to change their stand and allow the talks to move on towards a treaty with effective controls. On the same day the U.S. and U.K. jointly placed an item on the agenda for the coming 16th General Assembly of the U.N. (scheduled to convene in September) entitled "The Urgent Need for a Treaty to Ban Nuclear Weapons Tests Under Effective International Control."

The administration had been under pressure from various quarters right from the inauguration to break the deadlock at Geneva.

> **As early as February, the Joint Chiefs of Staff had urged the President to resume testing if agreement were not reached within 60 days of negotiations. The Joint Chiefs favored atmospheric testing. The Department of Defense, though, would have limited the resumption to underground testing. There were also pressures from Congress, especially from the JCAE, from the press, and from public opinion. A Gallup poll in July 1961 showed more than 2 to 1 public support for the United States unilaterally resuming testing.***

On June 14 the Chairman of the JCAE, Representative Chet Holifield, appealed to the President to announce "within a few weeks" U.S. plans to resume testing and concurrently continue the Geneva talks. Holifield noted a suggestion that had been more and more frequently heard since 1960, namely, that the Russians might be conducting secret tests.

On June 12 the Commissioners heard a special briefing on the Geneva talks by Wilmot Hess, who had been in attendance there for eight weeks. It was Hess's opinion that if the U.S. was going to resume testing, the testing should begin before the

*A. Schlesinger, *A Thousand Days*, page 370.

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commencement of the disarmament negotiations on July 31. Chairman Seaborg stated that it would be extremely difficult to prepare for nuclear tests prior to August 1. Hess also reported some observations from Vincent Baker, a member of the U.S. Geneva delegation, who had gone to Geneva as an advisor during the summit meetings. Part of Baker's report had been on conversations at Geneva between Secretary of State Rusk and Soviet Foreign Minister Gromyko.

> Mr. Gromyko admitted the "Troika" principle did imply a veto power. When Mr. Gromyko asked Mr. Rusk if he thought the Russians were conducting nuclear testing, Mr. Rusk said this was difficult to know and that a negative proposition was always difficult to prove. Mr. Gromyko said the Soviets are convinced the Americans do not believe the Russians are testing nuclear weapons.

In recalling the visit of Representative Holifield and Senator Hickenlooper of the JCAE to Geneva (May 24 and 29, 1961), Hess stated that:

[redacted]

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Hess also commented on a telegram which the Geneva delegation had received from the State Department, which included a draft of a letter from Mr. McCloy to the President, containing the following points for the delegation's comments:

> (1) The U.S. should resume nuclear testing because of the current unenforced moratorium; (2) the decision to resume testing should be preceded by a specific announcement a short time before the first test; (3) a general statement should be made by the U.S., possibly in concert with the U.K. and France, opposing atmospheric tests; (4) Mr. Arthur Dean should be recalled from the conference after a reasonable period of time, to emphasize the U.S. intention to place the conference on a lower priority basis; (5) the test ban conference should be merged with the general disarmament conference to begin in August; and (6) the U.S. should publish a statement at the time of test resumption that any nuclear test conducted by the U. S. would not result in a world health hazard.

In this atmosphere of heightened possibility of test resumption, the Committee of Principals met on June 16. At that meeting AEC Chairman Seaborg agreed to collaborate with Secretary of Defense McNamara to prepare a paper on weapons testing. The detailed planning and preparations requested by the AEC to prepare for testing, discussed by the Commmission on June 20 and also discussed at the meeting of the Joint Committee on Atomic Energy the next day, were based on a different basic assumption than that of the DOD's parallel efforts. That is, the AEC was assuming the President would announce that preparations for underground testing were under way, whereas the DOD was assuming that no announcement would be made and that the preparations would be done in the present climate.

Another expression of the pressures on President Kennedy appears in Theodore Sorensen's book, *Kennedy*:*

> Ever since he had taken office, Kennedy had been pressured to authorize a resumption of U.S. testing. Renewed American testing, according to the military and the Teller wing of the scientific community, was indispensable to

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*Theodore C. Sorensen, *Kennedy*, Harper & Row, New York, 1965, pages 617 ff.

-SECRET-the development of new nuclear weapons. It would provide a necessary hedge against the possibility that the Soviets were secretly testing underground. The Joint Chiefs urged him in February to resume testing if no agreement could be reached after 60 days of negotiations. . . . They were for atmospheric testing; the Defense Department was for underground testing; the State Department was for putting off a decision; and a variety of nuclear scientists said that no agreement was in sight, the moratorium had dangerously slowed our technical progress and the U.S. should test while continuing to talk. Similar pressures came from various parts of the Congress and press. Dr. Teller maintained publicly that the Soviets had been testing underground steadily since the moratorium began. . .

At the end of July, Khrushchev told McCloy that he was under strong pressure to test, especially from his scientists, and that the Berlin crisis had increased the pressure. He had been successful thus far, he said, in holding off the decision, but the more the U.S. intensified its threats of war, the more arguments it gave those in the Soviet Union who wanted to resume. His scientists favored a 100-megaton bomb as the most economical and, though they already had the rockets to lift it, the bomb itself needed to be tested. He had cheered his scientists, he said, by telling them that the U.S. would resume testing and thus release them to try out their own bomb.*

The September 1961 issue of the *Bulletin of Atomic Scientists* cited related statements made during the summer as follows:

- President Kennedy--Unless the Soviet Union becomes more cooperative, the U.S. "will probably have to begin testing--not right away, but sometime in the future."
- Seaborg---There is not convincing evidence that the Russians have been testing, and most American scientists think it unlikely.
- Hubert H. Humphrey-- The U.S. should not resume testing immediately: "The Soviet Union wishes to drive us into nuclear testing so that she may freely test."
- Henry M. Jackson, Chairman of the Military Applications Subcommittee of the Joint Committee on Atomic Energy--"we are running the serious risk of being outstripped in weapons technology tests. The possibility----- calls for immediate action for resumption of testing."

Another opinion was:

- Commissioner Robert Wilson (7/5/61) to Seaborg--The matter of overriding importance to the nation's safety----is the resumption at the earliest possible moment of underground weapons testing.

The President addressed the problem by appointing a special scientific panel tasked to review the problem of detecting and identifying nuclear explosions as well as to address the question of what we knew and whether we could know if the Soviets had been conducting clandestine tests. The panel, announced by Kennedy on June 28, 1961, was chaired by Prof. Panofsky and known as the "Nuclear Test" or "Panofsky" Panel. The members of the Committee, other than Panofsky, included Bethe, Bradbury, Fisk, and Foster. On July 14 Hans Bethe circulated a draft report which included:

The general conclusion reached by the Panel is that none of the specific weapons tests are of such urgency from the technical and military point of view that a delay in reaching a formal decision would be critical. The panel also believes that the Soviets may be under considerable pressure to resume nuclear testing in order to develop a mobile strategic deterrent. On the other hand, the Panel feels that in the absence of adequate progress toward a satisfactory treaty, it would be undesirable to let those parts of modern weapons technology dependent on testing stagnate while all other fields of military development proceed unhampered. Specifically, the Panel believes

*A. Schlesinger, *A Thousand Days*, pages 452 ff.

that it would be technically unwise to permit the moratorium to proceed indefinitely even for an additional period comparable to that already lapsed. As an overall conclusion, the Panel believes that political rather than technical considerations should determine decisions concerning the resumption of nuclear tests in the near future.

Norris Bradbury and Carson Mark wrote separately on the subject to Presidental advisor Wiesner on July 17. Neither believed that the Russians had been secretly testing and Mark felt that if they had, the rather low yields attainable could not have permitted important changes in strategic capabilities. Neither felt that there was a strong military urgency to resume weapons testing. They still did not see any great potential advances if tests were resumed; perhaps a factor of two increase in yield per pound of higher-yield weapons and some increase in efficiency for smaller weapons. Neither saw the development of a neutron bomb as likely. They saw substantial gains possible by high-altitude effects and vulnerability tests, but neither felt that there was overriding importance to early testing in these areas since the other side was equally limited by their ignorance.

On July 28 the Secretary of Defense sent to Mr. McCloy a recommendation that the Committee of Principals propose to the President that the U.S. initiate weapons test preparation.

On August 9 the Russians restated their unchanging position in a note to the U.S. which said in part:

It is evident that the efforts of the U.S. are aimed mainly at actually legalising the holding of tests in any agreement, if such was signed, and creating an International Control Agency which would be a pliant tool in the hands of the Western powers and would be used by their general staff to collect required intelligence.

They further stated that the Western position made it impossible for the Soviets to sign an agreement and that the West would have to bear responsibility for that.

The Panofsky Panel on Nuclear Testing met with both President Kennedy and the National Security Council early in August 1961 to report the results of their deliberations. Among their conclusions were:

It was feasible for the Soviet Union to have conducted secret tests, that there was no evidence that it had done so (or had not done so), and that there was no urgent technical need for immediate resumption by the United States. Oddly enough, the Livermore scientists, who a year earlier had discoursed most eloquently on the ease and convenience for the Soviet Union of testing in secret cavities underground, were now most insistent in proclaiming the inadequacy of underground testing for the U.S. and demanding that we go into the atmosphere as soon as possible. Foster argued vigorously to the President that immediate resumption was necessary in order to develop the neutron bomb. . . .The President remarked that he had understood that atmospheric testing was not indicated for the neutron bomb for at least another 18 months.*

President Kennedy was not convinced of the advantages to be gained by unilaterally resuming testing. He posed the hypothetical situation that the Soviets were not clandestinely testing, and that the U.S. resuming testing underground would result in the Soviets resuming testing in the atmosphere. He asked each Panel member if he would favor unilateral underground test resumption under the hypothetical situation. Panofsky answered no, Foster answered yes, and Bradbury answered no, adding that the Soviets could overtake us if they tested in the atmosphere while we restricted

*A. Schlesinger, *A Thousand Days*.

ourselves to underground testing. Following this exchange, McCloy noted the coming meeting of the U.N. General Assembly, feeling that the decision on test resumption could be postponed to the first of the year without impairing national security. Schlesinger* wrote:

> I came away with the feeling that, while there was no irresistible short-run case for resumption, everyone regarded a return at least to underground testing as inevitable in the long run if the Russians continued to reject the treaty. Kennedy wrote Macmillan early in August that he was still reviewing the evidence, but was not very hopeful that it would be possible to wait much beyond the first of the year. If we did resume, it would be underground, unless and until the Soviets resumed atmospheric tests.

Kennedy mentioned to Macmillan the idea of trying once again for a limited test ban agreement. Ambassador Dean resisted any retreat from pushing for a comprehensive treaty and:

> When the matter was brought to the President, he readily came up with a compromise--that Dean should fight for the whole treaty in Geneva, but, if nothing happened, we would come out for the limited ban later. In mid-August the President concluded that when Dean returned from Geneva and DOD had completed its review of weapon requirements, the AEC might announce contingency preparations for underground testing, though this would not mean that we had actually decided to resume tests.

The Warsaw Pact nations met in Moscow on August 13 and 14 and accused NATO of using Berlin as an espionage center. Furthermore, they proposed that East Germany "establish such an order on the borders of West Berlin which would securely block the way for subversive activities against the socialist camp countries" until a German Peace Treaty was concluded. Acting on this statement, East Germany then closed 68 of 80 crossing points along the intracity border to traffic from east to west and moved tanks to the borders to enforce the closures. Calling the incident a threat to world peace, the United States, the United Kingdom, and France, on August 17, condemned the closing of the Berlin border and called on the Russians to end the harassment. On August 18 President Kennedy ordered reinforcements for the U.S. garrison in West Berlin.

Theodore Sorensen's view of what happened during August is that:

> Finally, early in August, despite a new recommendation from Maxwell Taylor and the Chiefs that testing be resumed immediately, he (the President) decided to order preparations for underground tests but not actually to resume them until it was absolutely clear--not only to him, but to the world--that he had done everything possible to obtain a treaty, that the Soviets had not bargained in good faith or really wanted such a treaty, and that the security of the free world required this country to test.

In a further effort to break the deadlock in Geneva, President Kennedy made one last attempt at negotiations by having the chief negotiator, Ambassador Dean, return to the talks at the end of August to present another U.S. proposal. On August 28, Ambassador Dean offered two possible new treaty proposals, the first of which was a slight modification of the April 18 version. The second proposal was more far-reaching in that it offered not a threshold but a comprehensive test ban under which it might be possible to reduce or even eliminate the threshold immediately upon signing the treaty. In order to reach these latter positions, it would be necessary to reexamine the technical aspects, and such a step might be possible by increasing the number of control posts or the number of on-site inspections, as well as by

*A. Schlesinger, *A Thousand Days*, page 459.

making other technical improvements in the control system. The Soviets gave an immediate response that was totally unyielding, stating that a test ban agreement might have played a useful role as a first step toward disarmament sometime in the past 2 1/2 to 3 years, but that now the Soviet Union could only regard control measures as a screen for Western intelligence operations, and that now the test ban question could be solved "only in conjunction with that of disarmament." The next day, in Russia, an announcement was made that in light of growing international tensions and the Western military threat, Russia had decided to extend the service of certain of their soldiers who had been due for release from active duty.

On August 28 the Soviets broadcast an aircraft warning to stay out of a designated area over Siberia, an indication that they were preparing for atmospheric testing. Their actual announcement came on August 30 in a radio broadcast. The Soviet government denounced the Western arms and military buildup, saying that the West was "resorting to threats . . .to unleash war as a countermeasure to the conclusion of a peace treaty (Soviet) with the German Democratic Republic." Thus, the Soviets had:

> Made a decision to carry out experimental explosions of nuclear weapons. . . . It is an open secret that the U.S. is standing at the threshold of carrying out underground nuclear explosions and only waits for the first suitable pretext to start.

Further, the announcement noted that France had "conducted explosions of nuclear devices one after another," while the Soviet Union had refrained and that this would have put the U.S.S.R.:

> . . .in an unequal position as compared with the U.S., Britain, France, and other countries which are their partners in one military bloc.

The announcement also referred to the Soviet's arsenal of device-carrying rockets and designs for superpowerful (as great as 100 megatons) bombs.

When handed the information from the Russian test resumption announcement, Kennedy's

> . . . first reaction is unprintable. It was one of personal anger at the Soviets for deceiving him and at himself for believing them, for their tests had obviously been under secret preparation even before Geneva and throughout the Geneva negotiations. His second reaction was one of deep disappointment--deeper, I believe, than that caused by other Soviet action during his tenure.*

The Soviet announcement was publicly condemned over the next day or so by the United States, India, Japan, the United Kingdom, France, and West Germany, among others.

[redacted]

Withheld Under
5 U.S.C. 552(b)(1), Exemption 1
1.3 (a)(5), Dept. of State (DOS)

*T. Sorensen, *Kennedy*, page 619.

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Withheld Under
5 U.S.C. 552 (b)(1); Exemption 1
1.3 (a)(5)
DOS

New Test Planning, Mid-1961

Reeves' May 10 meeting (previously discussed) to begin a new readiness plan seems to mark an upturn in readiness interest and actions throughout most of the test system. LRL had been pressing for increased readiness-to-test effort all the time, but the real reason for the upturn was probably the fact that the Russians at Geneva had turned down President Kennedy's first attempt at tabling a complete treaty; they still insisted on the Troika, which Kennedy had branded on May 5th as unacceptable. The GAC had written to the Commission on May 2:

> The possibility of a breakup of the Geneva negotiations requires that the AEC be ready to resume weapons testing. We have had weapons development arrested for 30 months, and we should be prepared to initiate tests as soon as possible after the date on which permission might be given by the President. The underground technique in Nevada should be used first, and a program for this technique should be carried to within a few days of firing time.

On May 5 Seaborg had written to McCloy, the President's advisor on disarmament, that in the event the President should find it necessary in the light of the Test Ban Conference situation to decide that the United States must resume nuclear detonations and should make such announcement,

> The AEC recommends that . . . the U.S. begin underground detonations for the Vela Uniform seismic research program and resume nuclear weapons testing underground. . . . We believe that it would also be desirable to reserve the right to carry out, in the future, tests in other environments which would not produce worldwide fallout.

Seaborg then gave his recommendation for immediate nuclear weapons tests [redacted]

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Betts informed the Laboratories on May 11 of the contents of the Seaborg to McCloy letter, to which Bradbury responded vehemently on May 18th:

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> Unfortunately, we do not entirely understand the basis on which the potential list of experiments was selected and, therefore, find it impossible to concur that the list proposed which involves only one LASL device (at that, apparently only on an alternative basis) is satisfactory or acceptable.

After noting that the optimum listing of potential nuclear weapons tests depends upon a number of factors, such as the length of time that testing is expected to continue, the political importance of resuming testing as soon as possible, the amount of preparation that DMA would allow, and the ground rules regarding containment, he remarked that LASL had five devices on the shelf for immediate tests, if the facilities permitted.

> All that would keep them from being tested at approximately two-week intervals is lack of suitable facilities. This could be remedied by the following immediate steps: (a) procurement of suitable coax cable and other necessary electronics gear for alpha measurements by EG&G (six weeks to three months); (b) deepening existing holes to 800 feet and 1,200 feet (eight weeks and twelve weeks); (c) agreement that existing tunnel and hole facilities are for general AEC use and not the exclusive property of one laboratory. LASL is procuring, within its own facilities, suitable containing canisters for these experiments on a 30-day available basis. In brief, we are doing everything we can to get ready for testing short of getting ready for testing in Nevada!

He remarked that the devices listed did not reflect the longer-range interests of the Laboratory, but were important in that they could be done first. He went on:

> In general, we would regret seeing ourselves limping along "ten weeks after authorisation," "six months after authorisation," "one year after authorisation" and so on if what has been so widely described as the urgent need of returning to testing is correct. There seems to be enough things going on in connection with Vela Uniform or Plowshare that it ought to be possible to deepen a few holes and get some electronics ready under this convenient umbrella. Why are we being so difficult about the situation--now and a year from now? It should also be obvious to you in light of all the foregoing and in light of LASL's past and future stockpile contribution that we strongly object to your selection and division of tests. Taking any individual test series you will find out that appreciably more practical and stockpileable weapons have resulted from the tests conducted by the LASL than any other Laboratory. For all the reasons given we will not be able to support your suggested program when reviewed by the Commission or the Congress.

The same day, May 18, Betts asked Foster and Bradbury to work together on the designation of the most needed tests and to consider joint use of the NTS resources wherever it might be appropriate. Futhermore, he directed Hertford on May 23 to retain his mining capability, charging it to Vela Uniform on a nominal five-day workweek schedule, even though some of the work would be applied to maintenance of NTS weapons test readiness.

On May 19 Los Alamos working groups came to a number of conclusions concerning the canister design, including provisions for alpha and prompt sampling. On May 23 Ogle sent Bradbury the test division suggestions for the five-shot program on the basis that underground testing might be requested at any time but would cease on October 15. He discussed options for the moratorium ending June 15 or July 15. In both cases the use of a tunnel site would be necessary and the data would be minimal.

On May 20 Chuck Violet of LRL gave Reeves the list of desired improvements in the Area 12 tunnel complex, including the amount of work it would entail for the tunneling crews. Foster answered Betts on May 26, giving his proposed shot list and ready dates, with the very earliest being a minimum diagnostics test possible on June 22. He also requested information on whether planning would be for a definite window or indefinite period.

On May 25, in commenting on the minutes of the March inter-Laboratory meeting, Betts stated:

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> **However, we must be careful not to permit ourselves or our thinking to become too completely "conditioned" to a "no-test" environment. When and if the moratorium is lifted, as there is at least a reasonable chance that it will be, I would like to see us work out a more deliberate and orderly overall pattern within which we should conduct our future weapon development program.**

On the same day Betts reviewed the situation for Luedecke, stating that device availability was not pacing so much as site availability, diagnostics, cable procurement and installation, and contractor technical support. He recommended instituting procurement action, construction, and scientific installations as required, and asked for concurrence. In order to achieve an underground readiness posture of a few days, it would be necessary to have a detailed plan of laboratory and field activities; long lead-time purchases; major construction of holes and tunnels; and a substantial increase in spending. Betts commented that:

> **Any disclosure of such activities might indicate the U.S. is not negotiating in good faith at Geneva. In-house (quiet and less extensive) preparation seems to give readiness of a few weeks.**

All of this fussing resulted in a coordinated program which was sent, on May 26, from Hertford to Betts. Hertford noted that LASL wished to use the 15a granite hole at a depth of 950 feet, to deepen one of their 500-foot holes to 800 feet and one to 1,200 feet, and to ream out the 15b hole to a diameter of 36 inches, at a total cost of $455,000. The LRL proposed program would have some ten different projects and would cost about $2,500,000. Hertford recommended that the LASL program be authorized in its entirety and that the question of the assignment to LASL of the b.03 tunnel site and the 15a complex be included in discusssions at a general program meeting to be called by DMA. This meeting, which he requested, would include ALOO, LASL, LRL, DASA, and DMA representatives,and would establish the appropriate priorities for test preparations. On June 1, after talking with Seaborg, Luedecke okayed the proposed limited actions, but emphasized that care should be exercised to minimize the number of people involved.

At the June 1 meeting of the LASL Weapon Working Group, as further preparation for NTS testing, [redacted] Charles Browne discussed the prompt sampling techniques to be used on Orchid, compared them with the Livermore system, and pointed out other underground sampling possibilities suggested by investigation of the melt from some of the 1958 underground Livermore shots.

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On June 9 Reeves arranged to get the details of the LASL hole proposals to H&N for a feasibility study and issued authorization to bill this work to the Vela Uniform program. He went on to authorize REECo, on June 15, to begin work as soon as possible on the LRL tunnels and to also bill the work to the Vela program.

The Principals agreed on June 16 that Seaborg and McNamara would prepare a paper on nuclear weapons testing. To assist in the preparation of that paper, Betts sent to Luedecke on June 20 a paper

> **. . . to determine the advance preparations for underground nuclear weapons tests required to improve the Atomic Energy Commission's capability to respond promptly to a Presidential directive to resume nuclear weapons testing.**

He commented on several aspects of test preparation as follows:

- **NPG test readiness was presently inadequate to begin testing on short notice,**
- **In accordance with the directive from Luedecke on June 1, limited actions were being undertaken,**

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- **The June 20 proposed test list had been coordinated with all agencies and contractors except the DOD.**
- **He requested immediate diversion of $3,000,000 from Vela Uniform to begin construction.**
- **He recommended against public announcement of test preparations.**
- **He recommended that diagnostic equipment procurement and installation be initiated, the most critical requirement being coax cable.**
- **He recommended that DMA and the DOD develop a long-range test plan.**

The total cost of the short-range program, if conducted, would be $25,000,000 to $30,000,000 above current funds. The shots would be fired only when there was assurance that radioactivity would be limited to the Nevada Test Site if venting occurred. The proposed short-range test program, in possible order of detonation, was as shown in Table XII.

Betts appended a list giving the yield, description, and purpose of each of the devices in the proposed short range program along with a map showing test site availability. As he had commented to Foster the day before, there was no consideration of low-altitude atmospheric shots, underground shots that might vent, or Plowshare shots. On the same day, with Foster and Bradbury present, the Commission discussed and generally approved the DMA plan, evincing also great interest in the Owl concept.

During June Reeves raised the question of whether or not Vela Uniform preparations should be pursued independently of weapons testing. He requested a code name for the operation. Betts continued to stress that confidentiality was important and stated that he could see no quiet way to drill the vertical holes for LASL. (JTF-7 was discontinued on June 30.)

In parallel with these AEC actions DASA was reviewing its nuclear test plans. In a letter of June 22 to the Joint Chiefs, Admiral Courtney Shands, for the Chief, DASA, noted that planning for weapons testing in the atmosphere and underwater had been stopped in January 1960 and, thus, no detailed plans could be given to carry out their needs in those media, but he pointed out that many of the military effects needs, such as radar blackout, kill mechanism, kill radii of defensive warheads, and

TABLE XII
POST-MORATORIUM TEST PROGRAM PROPOSAL

Device	Laboratory	Time Required after Authorization (weeks)

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DOE, Exemption 3

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effects of electromagnetic pulses on weapons systems, would be difficult to accomplish underground. He listed the two underground tests for which some work had been done:

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- Marshmallow was to be [redacted] effects, and had a 12- to 18-month readiness that might be reduced to 9 to 12 months on a crash basis,
- Hardhat (formerly called Lollipop) was to be a 5-kt detonation underground in granite at the NPG to study effects [redacted] and had a readiness time of 4 to 5 months.

He emphasized that worldwide fallout from past tests had not produced a biological hazard, and stated that the testing philosophy of the United States should allow tests to be conducted in any environment for which information is required, taking care to ensure that local fallout does not occur over inhabited areas. He also suggested:

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- A 2-kt balloon shot at 115,000 feet to study blast measurements and energy partition at high altitude, with readiness of 12 months;
- Four to six shots at altitudes from 25 to 1,000 kilometers with yields in the range of 250 kilotons to 1 megaton to study the D region blackout, fireball effects, x-ray effects, magnetic containment of trapped particles, and kill radii of warheads, with a readiness time of about two years;
- A series of three to five tests of low yield to study underwater and water surface effects, readiness time approximately two years;
- A series of three to four tests [redacted] to accurately measure local fallout of low-yield weapons over large land mass, study low-yield blast phenomena, and hardened target response in high-pressure regions, with a readiness time of about 18 months.

Finally, he recommended a weapons development series similar to that from the Laboratories with emphasis on the Owl, stating: "When actual test devices are available for tests, this program should take priority over all others."

On July 8 a group of reporters toured the Nevada Test Site and published stories stating that work was progressing on two tunnel complexes designed for underground nuclear tests. Thus, on July 12, Johnny Foster said to Betts:

> In view of the recent publicity regarding testing readiness, I believe that we should reexamine the status of the basic and scientific construction at NTS, giving particular attention to those items which would materially improve our immediate readiness posture. Inasmuch as the AEC public information activities at NTS have resulted in extensive press reference to readiness construction at the site, I believe that we should reduce our sensitivity with regard to these matters, etc.

On the same day Captain Brady of the DMA Test Office opined to Betts that this was simply more of the same kind of pressure that LRL had kept on DMA for the past two years, but that he didn't feel that scientific construction or installations should be agreed to at this time without first seeking top-level guidance, which had to this point only imposed restrictions on the things that Foster had proposed. Betts replied to Foster more gently on July 31, regretting that the recommendations could not be implemented and stating that DMA was continually examining the readiness posture and updating their information; but at the moment, their guidance was to do nothing which could be picked up by the press.

In mid-July Jim Carothers of Livermore introduced the "Christmas Tree" concept. This concept would produce a massive underground testing area centered around a deep, vertical shaft. Off from the shaft, at various depths, would go tunnels, the lengths of the tunnels being roughly proportional to depth, hence, the words

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"Christmas Tree." At the ends of the short, upper-level tunnels, small detonations could be contained, whereas large detonations would have to be at the ends of the lower-level, long tunnels which had appreciable overburden. Carothers' design in concept would allow detonations with yields up to 200 kilotons. He pointed out a possible area for this facility in the vicinity of Pahute Mesa where there was apparently at least 4,000 feet of tuff lying above the NTS water table, and requested that Reeves direct the USGS to immediately begin geophysical work in that region to produce the proper mapping. A week later, on July 20, he had refined the design to five working levels that would allow detonations of 5 kilotons, 20 kilotons, 50 kilotons, 100 kilotons, and 200 kilotons maximum yields, respectively, and suggested that the one facility, costing $25 M in construction alone, should be capable of handling over 60 detonations on each level. LRL had contacted USGS to determine whether a suitable site for such a facility existed within NTS. Reeves took the proposal seriously and transmitted it to DMA for consideration. LASL objected strenuously, feeling that this proposal would use up all the engineering effort available and require much more money than was available, and fearing that any accident, if this system were used, could do away with the usefulness of the whole, expensive array. By late August the USGS had made plans to carry out aerial geologic mapping and seismic refraction surveys in connection with the concept.

In mid-July Bob Newman of LASL requested that H&N study the possibility of casing Area 3 holes with concrete instead of steel. The result was that the holes could be cased with steel for about two-thirds the cost of concrete and would be done in 60% of the time of the cement-sheathed case.

At the GAC meeting of July 13 and 14 the AEC proposed list of weapons tests was discussed. Norman Ramsey commented:

> It is absolutely essential that the U.S. be prepared to conduct a first-rate test program. This would include the digging of holes, and also the placement of devices in those holes if there should be an advantage in doing that. Even if this activity should become known to the world, the Geneva negotiations would not be hampered, since the activity would simply convey a measure of restlessness.

5 U.S.C. 552(b)(3)
DOE, Ex. 3

Libby complained that the [redacted] had been placed in inactive status ever since it was completed three years ago. Hearing the discussion of the health of the Laboratories, Wigner pointed out that Los Alamos was apparently losing interest in weapons research. The committee agreed to recommend to the AEC that both Laboratories should be reminded that their primary mission was, and would continue indefinitely to be, weapons research, and that future support would depend largely on successes in the weapon field.

Betts continued to stall on any large obvious effort, but also continued to worry about the problem. In late July he asked Hertford for a detailed breakdown of the cables installed in tunnels and requested a quiet review of the current test readiness posture, including device availability, diagnostic instrumentation readiness, and the detailed construction requirements for the short-range test program.

On July 20 and 21 DASA briefed the newly formed McMillan panel on their tentative nuclear weapons effects tests, including an extensive list of shots addressing electromagnetic phenomena, fallout, and nuclear and thermal effects. The list was forwarded to DDR&E on August 3.

By July 25 H&N had completed their estimates of the time required to activate the holes requested by LASL, including 15a and 15b. The time and cost were somewhat higher than previously estimated.

In early August Colonel Anderson of DMA informed Chairman Seaborg of the estimated costs for various kinds of nuclear detonations. He gave $2,000,000 to $4,000,000 per shot for underground tests, including both AEC and DOD costs; above ground NTS shots about $1,500,000 per shot; above ground Eniwetok shots about $3,500,000 per shot, and outer space testing at $100,000,000 for the first shot and $10,000,000 to $15,000,000 for each shot thereafter. On August 7 Betts asked the Laboratories to propose a program for testing in outer space.

On August 11 Betts informed the Laboratory Directors and Operations Office Managers of the present status of the proposed nuclear test program, requesting that his paper be treated in the strictest confidence and that distribution be held to an absolute minimum. He commented on the possible advances that could be made by testing in the fields of weapon vulnerability, fusion and fission-fusion weapons, high yield-to-weight ratio strategic weapons, pure fusion weapons, and unforeseen discoveries. Basic program guidelines were divided into three categories: a short-term program, a medium-term program, and a long-term program. Short-term was defined as six months utilizing underground sites, but the long-term program might include deep space also. He also commented that the DOD Hardhat test would be included in the short-term program and Marshmallow in the medium-term program. [redacted] August 8 Foster requested that Betts authorize stockpiling of cables at the factory if it could not be arranged to have them delivered to NPG. Reeves made a similar request the next day. On August 13 Systems Command Headquarters asked General McCorkle of AFSWC for his recommendation concerning the effective date of discontinuance of the 4950th Test Group. The dam began to leak a little on August 17 when Betts authorized ALOO and the nuclear laboratories to proceed with readiness work, the cost not to exceed $3,000,000, which would be funded from Vela Uniform money. Work would follow the guidelines of the August 11 letter. He specifically authorized construction on the [redacted] site in U-12b.06 and completion of the work [redacted] U-12e.03a. He authorized construction of an alpha system for LASL to the tune of $160,000, and also authorized ALOO to initiate procurement of $1,300,000 worth of cable, stating that this procurement action should be carefully handled because of the somewhat greater possibility of speculation and public disclosure. He then asked for any further suggestions that the testers might have on test planning. He listed what he saw as the possible risks of public disclosure for activities that might occur. It is clear that at this time there was simply a decision to take a little more risk, but there was certainly no conviction that we would immediately begin testing.

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On August 30 the Russians announced their intent to resume testing.

The Moratorium Ends

Thus the moratorium ended, 34 months after its beginning. In those 34 months the U.S. had disbanded its organization for conducting overseas tests and essentially dismantled the site for so doing. A great number of carefully thought-out plans on how future overseas tests should be conducted were left. The AEC had convinced itself that further testing in the atmosphere would not be allowed. Following the growing conviction that underground testing was the thing of the future, some eight or ten sites capable of handling full-scale nuclear detonations had been constructed in Nevada under the guise of readiness, Vela Uniform, or weapons effects. Because of the growing introduction of efforts other than weapons tests to the Nevada Test Site, for example, Rover, Pluto, Plowshare cratering, and Vela Uniform, the NTS organization maintained appreciable strength; kept its contractors working, a radsafe and weather organization going, a timing and firing system operating; and, in general, maintained those capabilities necessary for testing. Unfortunately for weapon development testing the money for site construction during the moratorium was spent in the wrong place, that is, on tunnels instead of vertical holes. However, that experience was invaluable to the DOD as the beginning of development of those techniques later used for underground effects tests.

In a similar fashion, the Laboratories had, under one guise or another, retained sufficient test personnel to be able to respond quickly and even to advance the technological state of the art. Most of the old testing hands had somehow weathered the storm and stuck through that thirty four months.

Even though contractor effort at the Nevada Proving Ground was not enough to keep all of their people with test experience, some of the contractors, notably H&N and EG&G, managed to hold on to their old testing hands by using them elsewhere in their organizations.

Summation of 1961 Through August

Nineteen sixty one saw a new Administration with renewed hope that its new initiatives would result in Nuclear Test Ban agreement with the Soviets. It took about a month of negotiation for the new administration to realize that they had been overly optimistic, but Kennedy held on to his fading hopes until perhaps mid-August. In spite of the growth of conversation and "what if" questions during the year, very little real work was done on physical preparations to test until the Russians resumed testing, but the growing effort on test planning in the previous six months made the actual return to testing in September feasible.

CHAPTER III

RETURN TO TESTING--NEVADA

NTS Readiness Status

As the anticipation of test resumption grew during the months before September 1961, plans were refined and some preliminary schedules were defined. As an example of a short-term program, DMA circulated a plan to the Laboratories and Operations Offices on August 11 (see Table XIII), which suggested [redacted] tests to be performed underground at the NTS. The Laboratories were asked to gear their in-house activities to preparing for such a short-term program, if possible, and to maintain the flexibility to modify this program as politics and high-level decisions dictated.

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Responses to his August 11 program concept led Betts to the following summary, which was part of an August 17 paper on preparation for test resumption:

> At the present time, there exist two principal tunnel complexes at NTS, with approximately 20,000 feet of tunnel and side drifts for conducting underground tests. Also, there are three small tunnels. In general, LRL experiments would be concentrated in the tunnels. In addition to the tunnel systems, four vertical drilled holes, 500 feet deep, have been prepared in Area 3. These holes are presently suitable for tests having yields up to about 1 kt. In general, LASL experiments would be performed in the vertical holes, although provision has been made for conduct of a 40-kt LASL experiment in the U-12e.03 drift if so required. In addition to the excavation work, cable runs have been installed for Orchid--the proposed initial shot of the Vela Uniform series--consisting of 14 runs of 5,500 feet each . . . The present readiness position for underground tests, if and when it is decided to proceed, is in large part based upon the tunnel complexes begun during previous test operations, the cleaning out and improvement of these tunnels, and to an extent, work performed under the Vela program . . . The present condition of readiness to resume testing permits only a few (perhaps four) of the initial test shots to be done with reasonable assurance that significant diagnostic information could be obtained. There is no assurance that all types of cable required for the experiments are on hand or could be procured in time to meet all portions of the schedule. Based upon the most recent cable procurement action, delays of one to four months (beyond promised deliveries of one to three months) were encountered, and such types of delay must be anticipated . . . In order to allow for unknown factors or unpredictable delays, the initiation of certain actions now which would not unreasonably increase the risk of public disclosure will increase the probability of carrying out the entire short-range test program on the time scale indicated. These actions would include the extension of the current planning effort and the procurement of the necessary cable and additional diagnostic equipment. In addition, the degree of readiness achieved through undertaking these actions may contribute to some reduction in the present schedule proposed.

In the same paper Betts authorized ALOO and the Laboratories to proceed with readiness work consistent with his August 11 letter. (See Chapter II for details of the work authorized.) However, because of the risk of public disclosure, at the end of August no vertical hole drilling was authorized, and cable procurement, soon to become a very critical item, was still only in the planning phase. Actual cable procurement was to be some time off.

TABLE XIII
DEVICE, SITE, AND DIAGNOSTICS READINESS SCHEDULE
SHORT-TERM PROGRAM

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5 U.S.C. 552 (b)(3)
DOE, Exemption 3

5 U.S.C. 552 (b)(3)
DOE, Ex. 3

Real Preparations Begin

The long-sought words from high levels finally came, but only when they were inevitable, on August 31, the day after the Russians announced their intent to resume testing. Betts wrote: "We are authorized to proceed in the readiness program without the unusual security restrictions which have previously applied to our planning efforts in this regard." He also stated that something must be tested as soon as possible, setting September 14 as the target for the first test. He asked the Laboratories to comment on the August 11 short-term test program (Table XIII), coordinating their comments through ALOO within two weeks. Moreover, Betts said that the first test should be [redacted] and he stated that meeting the schedule was more important than the diagnostic information to be obtained. Bradbury reacted to this point on September 1 (before the first Russian test): His Laboratory's "senior staff opinion . . . is unanimous that this procedure appears insane and that for the U.S. to be mousetrapped by the U.S.S.R. into testing before the U.S.S.R. would be a national blunder of the first magnitude." Events quickly overtook this concern.

On September 1, Reeves sent a message to Fred Hohner of the AEC Las Vegas office authorizing that office to proceed immediately with basic construction of U-12e.03a in accordance with Livermore criteria, including extending the drift by approximately 150 feet. Apparently, the construction had not yet begun on the drift extension, and Reeves estimated basic construction would be complete by October 15. On the same day, Hohner sent a message to Carl Taylor of REECo and Frank Hines of H&N, both at Mercury, directing them to proceed immediately, but without causing too much publicity, with activities necessary to prepare the U-3ac hole for LASL use on September 10. Specifically, the hole was to be backfilled from 500-foot depth to about 250 feet and finished for device readiness.

The Russians' first atmospheric detonation in 34 months took place at Semipalatinsk Proving Grounds at 3 a.m. EDT on September 1, signaling the real end of the moratorium. [redacted]

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All of this occurred before a public Presidential announcement of any U.S. test resumption. What was going on in Washington in these crucial days? In the AEC, discussions among DMA, the Labs, Operations Offices, and contractors were aimed at identifying programs and various means of doing them. Luedecke sent a paper based on some of these discussions to Chairman Seaborg on September 4, setting forth several alternatives to the already-planned short-term testing program. The paper listed the disadvantages and advantages of each alternative. [redacted] While these three shots could be completed between September 14 and October 12, doing so would delay subsequent testing because virtually all of the existing cable inventory would be used, leaving little cable for further diagnostic data collection. The second alternative was to test a high-yield device [redacted] While this test could be done within a few weeks, there was uncertainty as to its effects on the remainder of the tunnel complex in which it would be fired, and it was felt that an additional four to five weeks should be taken to gain assurance that the device could be fired without jeopardizing the rest of the complex. Further discussion of alternatives addressed various techniques for atmospheric testing, including balloons, either overseas or at the NTS. Atmospheric testing at the NTS was believed to be possible within a few weeks, but the larger yield devices would have to be tested outside the continental United States, requiring an estimated minimum of three months preparation. Luedecke stressed that each of the alternative quick-response programs would cause problems in the long run, and he recommended that the short-range program already planned be implemented so as to permit maximum use of the available facilities and secure adequate diagnostic information.

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A letter from AEC Chairman Seaborg to McGeorge Bundy, the President's National Security Advisor, on September 5, disclosed that these alternatives had resulted from discussions several days earlier among Seaborg, the President, and the Secretary of Defense, Robert McNamara. McNamara had asked how the AEC could respond on a "two weeks, four weeks, six weeks schedule" reflecting the first alternative, and, observing that such small shots might not be noted other than by our announcements, he further requested a schedule for an initial shot of high-enough yield to be felt off-site by foreign observers. Seaborg, in his rebuttal to the suggested alternatives, not only repeated Luedecke's list of disadvantages, but added that "the

Soviets have tried hard to picture us to the world as having made continuous preparations for testing . . . to be ready to start at the very first opportunity. They surely would take advantage of any announcement by us of an almost immediate test detonation to further this line." Recommending to Bundy that the test program authorized be the short-range program under preparation since July, he asked that a decision be made on the same day, September 5, to enable the AEC to carry out the first detonation on September 14.

Presidential Direction to Prepare

How did President Kennedy reach his decision to resume underground testing? On the day of the first Russian test, buffeted by a variety of recommendations for action or inaction from his many advisors, he declined to announce a test resumption, but made very clear his feeling that he could not refrain much longer. On the 3rd, Kennedy joined with British Prime Minister Macmillan in openly offering Khrushchev an agreement to ban any atmospheric tests that would produce fallout, "pointing out such a pact could rely on existing means of detection and would not require additional controls."* The Russians ignored this and carried out their second and third detonations on September 4 and 5, whereupon Kennedy felt he had no choice but to allow U.S. test resumption underground, which he publicly announced on September 5. At the same time, however, he stated that the atmospheric test ban proposal would remain open until September 9. An interesting insight into Kennedy's personal anguish and resolve over this most important decision is provided by Schlesinger,** who recounts a meeting the same day on other matters with several top advisors, including Adlai Stevenson. Noting his fears that the Soviets might respond to the Kennedy-Macmillan note on an atmospheric test ban by bringing up general and complete disarmament and thereby "scooping" his own disarmament initiative, Kennedy expressed his own personal regret at the decision he had had to make this day: to resume underground testing.

> Kennedy quickly said, "What choice did we have? They had spit in our eye three times. We couldn't possibly sit back and do nothing at all. We had to do this." Stevenson remarked, "But we were ahead in the propaganda battle." Kennedy said, "What does that mean? I don't hear of any windows broken because of the Soviet decision. The neutrals have been terrible. The Russians made two tests after our note calling for a ban on atmospheric testing. Maybe they couldn't have stopped the first, but they could have stopped the second. . . . All this makes Khrushchev look pretty tough. He has had a succession of apparent victories--space, Cuba, the thirteenth of August [the Berlin Wall], though I don't myself regard this as a Soviet victory. He wants to give out the feeling that he has us on the run. The third test was a contemptuous response to our note. . . . Anyway, the decision has been made. I'm not saying that it was the right decision. Who the hell knows? But it is the decision which has been taken."

Pretty clearly, the President made the decision that he had earnestly sought to avoid and he felt very emotional about it. At any rate, the test community, pulling itself together as it raced ahead on an uncertain path, at least had firm direction and permission to do the work that was already in progress.

*A. Schlesinger, *A Thousand Days*, page 480.

**Ibid, page 482.

Towards the First Shot

5 U.S.C. 552(b)(3) DCE, Ex 3

Back in Nevada, at Reeves' request Hohner was coordinating the Livermore and LASL cable requirements in order to expedite a cable purchase which was still not under way in spite of the July and August discussions which had pointed out the urgency. At LASL, trailers were being prepared for shipment to the Test Site within the next day or two, and the radiochemistry group had approved the sampling configuration for the first shot. Ogle informed J-6 that Sandia would do some seismic measurements, that the first shot was to be on the 14th, and that a 625-foot hole for the second shot [redacted] was planned as soon as possible but not earlier than October 1. In line with this, work was started to deepen hole U-3ae to 800 feet (it was later used at 630 feet) and to start a 6,000-foot exploratory hole as soon as possible. On the 6th it was decided that the first hole would be backfilled with clean silt for a short distance above the can, followed by sand the remainder of the way up to within five feet of the ground, with a concrete cap on top. On the same day, Ogle requested that the USGS (United States Geological Survey) determine the locations of water tables between 1,000 feet and 6,000 feet depth. Arthur Cox, J-15 group leader, discussed with J-6 the possibility of making yield measurements by hydrodynamic techniques, which would involve drilling satellite holes.

The next day J-6 and H&N worked out a plan for preparing satellite holes up to 12 inches in diameter and emplacing gauges to be used for measurements such as shock time-of-arrival and pressure. The pace was hectic. The J-6 man at NTS reported an immediate need for 10,000 feet of a particular type of cable, to replace one that had been cut by a blade that morning. He also commented that he was "shook about Campbell's apparent order not to raid site 400 [Rover]* for REECo support." He was instructed ". . . to use what was necessary, all if required, but no more."

On September 5 Al Graves, in a letter to Jim Reeves, requested authorization to design a number of facilities for NTS testing in Area 3, including area and shot site layouts; vertically drilled, large-diameter holes of any depth with either concrete or steel casing; new head houses and towers; direct radiochemical sampling systems; coaxial cable feed and placement systems; canister and cable lowering harnesses and hoist arrangements; well backfilling operations and equipment; and new deep hole winches. Graves pointed out several advantages that would accrue by accomplishing design now.

> First, long lead time procurement items will be specified sufficiently to permit orders to be placed early. Second, the overall costs of an operation should be reduced through orderly design and procurement schedules and with a smaller probability of expensive field corrections traditional with crash operations. Third, the time until the completion of a test operation will be reduced by the time spent in design before such an operation is authorised.

In a second letter to Reeves, Graves expressed concern about the very deep test hole required by LASL in Area 3. The letter briefly laid out the need for geologic and other information at great depths and requested that a 6,000-foot core hole be drilled in Area 3 as outlined by J-6 in an attached paper written August 10. Illustrative of the confusion in the first few days, a September 6 message from Betts to Hertford and the Lab Directors stated that "The President has requested that a shot be fired on September 15 and that the following events (Ed. note; not included here) be conducted on an early and frequent schedule, subject to obtaining beneficial technical information. The President was advised that [redacted] could be readied by

Withheld Under 5 U.S.C. 552 (b)(3) DCE, Ex. 3

*"Rover" was the nuclear-rocket program, since defunct.

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September 15; please plan accordingly." That same day, perhaps before seeing Betts' message, Hertford sent a message setting out a brief schedule for Reeves and other AEC management personnel and contractors, with information copies to the Laboratories. Reeves' schedule showed the first two events [redacted] to be performed immediately after the first test. Some clarification of this confusion came in a message from Betts to the Lab Directors, Hertford, and Shute:

> I recognise that some explanation may help to explain my verbal instruction given Sept. 5 to proceed with a test [redacted] on Sept. 15 in contradiction to the program of testing which was discussed with your representatives at the meeting in Albuquerque on Saturday, [redacted] The Chairman hoped that yielding on one very early test would take the heat off the program of three events previously planned for a crash effort and discussed in Saturday's meeting. Our major goal was to insist that any tests after the first one can actually achieve the technical results desired by the testing laboratory. This position is now supported at the highest level. We consider the new instructions to be a real victory for our recommendation that we not be forced to do technical experiments before proper instrumentation is available to achieve the desired technical results. It was in this context that we agreed to a [redacted] test as more or less of a "sacrifice" event in that we knew that we could not obtain all the diagnostic information which we would desire. . . . [redacted] certainly we should go ahead with preparations to carry out that event expeditiously. The date of its firing will be specified from here. It is important for reasons other than technical that [redacted] must be the first device tested.

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The next day Betts informed the same addressees that a Commission letter was going to the President requesting authority for [redacted] on September 15, informing him that [redacted] could be ready on the same day,* and requesting authority for that shot. Betts' message contained precisely the same schedule as is shown in Table XIV, dated September 6, and while it did not clarify the origin of the list, it did establish that it was agreed to by DMA within 24 hours of the date given by ALOO.

On September 7, Bradbury sent Betts a message summarizing LASL's plans and philosophy in respect to underground testing. LASL recommended testing as rapidly as NTS facilities allowed, at depths determined by a 475 $W^{1/3}$-feet rule for overburden, where W is the yield in kilotons. Tentatively, LASL planned 12 to 16 tests for the first year, and the specific schedule set forth was essentially the same as that presented in Table XV. Bradbury said that groundwater and seismic effects problems would not be considered until they showed themselves to be problems. Vela Uniform experiments or other effects measurements were to be done on a catch-as-catch-can basis. The weapons testing philosophy, Bradbury felt, should "put maximum effort on immediate improvement or test of stockpile items which might be involved in a real dirty knockdown war-to-win with the U.S.S.R. and on extension of stockpile into small, two-stage areas utilizing smallest possible primary." Finally, it was the LASL opinion that atmospheric testing with balloons, barges, or airdrops was the best and quickest way of making progress and the only practical way of proof testing weapons in excess of 200-kt yield.

On September 7, Dale Nielsen, Livermore Test Group Director, sent Hertford a status report on their preparations for the Antler event. He reported that excavation of U-12e.03a was complete and that diagnostic equipment was being emplaced. The first signal dry run was scheduled for Saturday afternoon (September 9) and the device was to arrive the same day, with the first diagnostic dry run scheduled for September 12. The final and complete dress rehearsal was scheduled for September 14. Livermore expected Antler to be ready, as scheduled, at 10 a.m. on September 15.

*It was, in fact, ready on the 7th of September.

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TABLE XIV
SHORT-TERM PROGRAM--TENTATIVE SCHEDULE

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5 U.S.C. 552 (b)(3)
DOE, EXEMPTION 3

On September 8, Joe Sanders, ALOO Director of Logistical Support at Mercury, sent Hertford a status report on preparations for firing the first two events on September 15, noting that the field organization had attained the capability to fire both of them as directed on that day, and giving some details of the status of the two separate events in the two areas. For the LASL test in Area 3, the items remaining were the arrival of a crane to lower the device and the arrival of the radiochemistry sampling pipe, which was scheduled for installation on Monday, September 11. For the Livermore test in Area 12 the final blasting for the zero room was completed at 9 a.m. on September 8, and final mucking, installation of coax, and connection of tunnel motor generator sets were underway. The stemming material was scheduled to arrive on the 11th of September, with installation to begin after the first successful dry run. As for other site activities and projects, the Command Post and programmatic building had been opened and all the offices were manned; housing would be short during the weapons test periods; and preparations for DOD participation, to be housed in Area 12, were being scheduled on a noninterference basis and within REECo's capability to complete the desired construction.

In another important message to Betts on testing philosophy on September 9, Bradbury further explained LASL's opinion (incorrect, as it turned out) that it would prove somewhere between impractical and impossible to test at much beyond 100 kt underground because of the need for adequate containment, groundwater problems, and seismic effects. Bradbury said that LASL was not convinced at this point that good yield measurements could be made on underground tests because of the appreciable thermonuclear contribution (also to prove incorrect). In spite of this, he said, "If the yield measurement problem can be surmounted, it is probable that underground testing up to 100 kt or so can do everything except air effects experiments that can be done in the air." He added that in addition to the yield flexibility offered by atmospheric testing, as well as the capability to make better measurements, the time factors seemed to favor atmospheric testing because things could certainly be done more rapidly. In spite of the negative view of the relative advantages of testing in these two regimes, this is probably as generous a view of the future of underground testing as Bradbury had yet openly stated.

From Newman's (J-6) notes, the LASL plan on September 8 showed canister delivery from LASL to the NTS in time for a September 14 [redacted] October 1. [redacted] The high level of activity led to a mad scramble for cabling, connectors, and "Crosby clamps." Rea Blossom (J-6 at the NTS), reported to Newman that the drill rigs would be in place on the night of the 12th, and that EG&G planned a dry run of their diagnostic and firing equipment on the evening of the 8th. In discussion of the 6,000-foot core hole, to be positioned one-half mile west of instrument bunker (alpha station) 3-300, Blossom asked Newman if the so-called "Hi-Vac" system would be okay for recovery drilling in the event of a shot at this depth, and Newman told him "Yes, if debris can be contained and personnel protected." Newman had also checked that day with George Cowan, head of LASL's radiochemistry group, as to whether the "Orchid" hardware would be satisfactory for a possible test in U-12c.03b and Cowan said that it would be fine if that shot were done earlier than Orchid. The potential Orchid test referred to was one which DMA had asked LASL to consider, [redacted] in U-12e.03b in late December. Ogle met with Westervelt and Hoerlin on the possibility of Orchid in that period, and replied to Betts that this could be done but the overburden was 300 feet short of that required for containment by a 450 $W^{1/3}$ rule. LASL had been planning that shot for a 2,000-foot hole during the week of January 14, 1962. At the end of his notes, Newman gave the first indication of a new date for the first shot, noting that Art Cox had met with Bradbury, who had revealed that the first shot date was now September 15. LASL could, in fact, have been ready to fire its first shot on the 10th or 11th.

Khrushchev finally replied on September 9 to the Kennedy-Macmillan proposal, saying that a limited test ban would permit the West to improve their weapons by underground testing, for which they had been preparing, and that the Russians would

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not agree to such a "dishonest deal."

In addition to the LASL and Livermore discussions on underground and atmospheric testing, Hertford told Betts of his conclusion that--given certain assumptions--one or two atmospheric tests could be quickly staged at the NTS without too much consideration for public opinion because of the attitude of the people in that geographical area, among other things.

On September 9, Reeves, the NTS Test Manager, published the operations order for Nougat, the nickname given to these underground tests. The organizational structures to conduct these underground tests included the following key personnel, in addition to Reeves: Air Force Colonel Leo Kiley, military deputy; Alvin Graves, scientific advisor; Roger Batzel, alternate scientific advisor; Dale Nielsen, Livermore test group director; Lieutenant Colonel John Kodis (DASA), DOD test group director; and William Ogle, LASL test group director. (DOD involvement in NTS field testing was the responsibility of the Weapons Effects Test (WET) section of Field Command, Defense Atomic Support Agency, headed by Colonel Kiley.)

On September 6, representatives of ARPA and WET had met in Washington to discuss the impact on Vela Uniform of the projected usage of NTS tunnels for weapons tests and to define any necessary revision of Vela Uniform program plans. Betts sent a message on September 9 to the AEC organizations, ARPA, DASA, and the Air Force, giving proposed plans for continuation of Vela Uniform work as resources became available, based on working in conjunction with the weapons testing program and without undue interference with it. On that same day, in a message to Hertford at ALOO, Betts emphasized that first priority was now to go to weapons testing and that the following instructions applied to Vela Uniform events: Shoal and Porpoise were to be placed in an inactive status; Linen was canceled; the Orchid goals could be fulfilled by the Livermore [redacted] test; Stingray and Lollipop requests could be fulfilled by the Livermore [redacted] test and Hardhat, respectively; Crystal and Muslin requests would be partly addressed [redacted] the Plowshare Gnome shot was now being planned to include Vela Uniform objectives; and the use of Area 3 for Vela Uniform purposes should be investigated since this would give information on a new medium.

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Meanwhile, back at the NTS, Ogle's diary records that he came upon Jerry Tatom at 8 a.m. on September 9 placidly surveying his nice, neat zero area with a big smile on his face. Some hours later, at 3 p.m., 100 people were milling around the area in the dust. The next day, the alpha/reaction history rack and the device canister were joined together by 3 p.m., and dry runs continued well into the evening to determine whether the scheduled work could proceed. Discussions with USGS on September 10 led to a decision to put the first 2,000-foot hole close to Well 7, where the water table was at considerable depth. There was also a tentative decision to drill a 6,000-foot hole in granite monolith approximately two miles northeast of Area 15, where the geologists were confident (but not positive) there would be no water problem. Ogle concluded that this "obviously means that a portable alpha station is necessary."

A September 11 memorandum from Hertford to Reeves specifically appointed Reeves Test Manager for Operation Nougat and redelegated to Reeves the authority given to Hertford by DMA. In this role, Hertford told Reeves that he should report directly to the Director of DMA for operational matters pertaining to Nougat. However, for normal administrative actions he would continue to be responsible to Hertford. Among the specific directives given to Reeves as Test Manager, in addition to his overall responsibility for the operation and for meeting the technical needs of the AEC Laboratories and the DOD, was the responsibility to emplace the devices so that "as far as can be predicted within existing experience and capabilities, containment of the nuclear detonation" would be ensured. Specific rad-safe criteria to be met by the Test Manager were also laid out, including ensuring that external whole-body

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radiation exposure to persons off the NTS would not exceed 3.9 rem* for any calendar year. Reeves was to designate September 5 as the effective date for the commencement of the operational period of Nougat. As of September 11, only three shots [redacted] had been authorized for execution. The specific schedule attached by Hertford (given earlier as Table XIV) was established on September 6, although the memo does not clarify how it was to be accomplished.

On September 11, Jim Reeves authorized Bill Ogle to emplace the [redacted] device.

Presidential Approval to Resume Testing

The President made his decision for test authorizations on September 11 or 12. He specifically approved the first three events, with the Livermore event first on September 15 for the political reasons cited earlier (external visibility), and the LASL event on September 16 since he did not want two tests done on the same day. This latter point was not made clear until late on the 12th of September. That morning LASL had lowered their device canister to the bottom of the emplacement hole and, after a discussion with Bradbury, decided not to backfill until the last minute in case there was trouble. It was that afternoon that Bradbury got word from the President that LASL couldn't fire until the 16th: in Ogle's words "Bradbury was hopping mad."

Minutes of the AEC meetings indicate that the Commission did not discuss test resumption in any detail until their September 13 meeting, during which Betts briefed the Commission on the President's decisions and the proposed short-range program of 15 tests to be done by mid-February 1962. Betts also presented a suggested draft of the request to the President for authorization to expend the special nuclear material for the first 15 tests, should the Commission approve the short-term program. Discussion of uncertainties about the 15 tests led to a decision to clarify the letter to the President, requesting permission for the Commission to make minor program changes as they deemed necessary, without new Presidential authority. In discussion of the forthcoming (September 14) Congressional hearings on the pros and cons of underground and atmospheric testing, Mr. Ink (AEC staff) opined that the question would arise of U.S. gains or losses vis-a-vis the Soviets should we restrict ourselves to underground testing. Betts noted that underground testing was approximately twice as expensive as atmospheric testing and that although the U.S. would be limited in the size of weapons it could test underground, he felt that tests of devices with yields in the megaton range would not be precluded. The possibility of a third kind of testing, underground testing that would not be fully contained but would have controlled venting, was also briefly discussed. Betts informed the Commission that preliminary discussions with ARPA, AFTAC, and DASA made it appear that major portions of the planned Vela Uniform data could be acquired by instrumenting the planned Nougat series. However, with the possible exception of the DOD Hardhat test, Vela participation would be on a noninterference basis. Chairman Seaborg said that nongovernment U.S. seismic stations would not be alerted for the first test and probably would not be able to detect the second test, but would probably be prepared for the third shot. He requested that appropriate seismologists be invited to participate in a seismic research program beginning with the third test. As for budgets, Betts estimated AEC costs for the planned series of 15 tests at $35 million, with an additional $20 million in costs and $18 million in commitments for FY 1962 preparations aimed at an FY 1963 program. An example of such preparation would be $6

*Roentgens-equivalent-man, a measure of biological dose.

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million required immediately for necessary engineering and surveys of a new tunnel complex and deep holes for high-yield tests. Thus, the total requirement for FY 1962 testing was estimated to exceed $80 million. Congress, he said, was making available $30 million, and since it was not clear that the $30 million would be sufficient to conduct tests through February 1962, approval should be given for DMA to reprogram funds on an interim basis. The objectives as stated by Betts for the first 15 tests would include improvement in yield-to-weight ratio in tactical and strategic weapons; examination of effects, including warhead kill mechanisms for ABM systems and "hardness" estimates for underground missile sites; and examination of the effects of U.S. warhead vulnerability vis-a-vis enemy ABM systems. After this discussion the Commission approved, with certain minor changes, the 15-shot program and the special nuclear material request to the President. They noted that the procedures for informing the President of program changes would be discussed between the Chairman and the President and that, after Presidential approval, the JCAE would be informed of the test plans by letter, and a directive to the General Manager to carry out the Nougat program would be issued. It was also noted that supplemental funding from Congress would be requested to support testing beyond the first 15 tests and for additional preparations for subsequent testing.

After a hiatus of more than two years, the Nevada Test Site Planning Board held a meeting at the Test Site on September 13. They had plenty to talk about, as indicated by the agenda (Table XVI). Reporting to Jim Reeves, the NTS Test Manager, and chaired by Al Graves of LASL, the membership included Bill Ogle of LASL; Duane Sewell and Roger Batzel of Livermore; Leo Kiley of Field Command-DASA; Robert Corsbie of Headquarters, AEC, Civil Effects Test Office; John Eckhart of Sandia; and Bob Miller of ALOO (Secretary).

Early on, Colonel Anderson of DMA, in response to a question about the decision on which tests to do on which days, stated that these decisions had come straight from the President and probably had some political basis. In subsequent discussions, the Board considered the shots proposed by the various agencies and developed a schedule of events including a short-term program of 15 tests running through February as well as a mid-term program extending into September of 1962 (Table XV). They recommended that Presidential approval for the special nuclear material to be expended in Nougat be in gross figures rather than related to specific events. LASL stated that after the first four events they were basing their planned activity on a concept of two events per month. Device readiness would influence the schedule, as well as site readiness. In addition to the weapons test program, it was felt that two Plowshare events per year could be accommodated in side-drifts of the U-12b, e, or g tunnels as a rather small addition to the schedules, and that the Plowshare "Wagon" event could be accommodated in Area 18. As for DOD experiments other than Hardhat and Marshmallow, the philosophy was now that they would not be considered in an integrated fashion by the Planning Board, but would be approved in Washington and integrated into the schedules at the field level. Lengthy discussion addressed the question of where to carry out LASL's [redacted] test. It was agreed that U-12e.03b would be returned to Livermore and should not be used for a large shot, but that one of the other large-shot sites in the tunnels (e.07 or e.06) might be available if it turned out the [redacted]

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In addressing the Vela Uniform program, it was decided that the Linen site (U-12b.07) would now be designated as a low-yield weapons test site, Vela Uniform programs would be integrated on a noninterference basis with other tests, and the DOD Test Group Director would work with each Laboratory Test Group Director to coordinate Vela Uniform activities. The group decided that any needed "care and feeding" of the

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8 million pounds of high explosives stored at the Test Site for Linen was the responsibility of the ALOO office of field operations (OFO). It was noted that, as stored, the HE was not an immediate safety problem, but since it was not to be used for Linen, it should eventually be disposed of, perhaps being used in the DOD Groundhog program (which might utilize up to one and a half million pounds) or by various other potential customers.

TABLE XVI
AGENDA
MEETING OF NTS PLANNING BOARD
September 13, 1961

1. Review DMA-LASL proposed events and schedule. If possible, integrate LRL items.
2. Consider DOD proposed list of experiments.
3. What constitutes the short-term program?
4. Consider and recommend emplacement of LASL [redacted]--U-12e.03b or deep-hole Area 3.
5. What, if any, policy is recommended for integrating the Vela Uniform program?
6. Consider disposition of HE (Area 2)-8,000,000 pounds.
7. Consider immediate and longer range list of support facilities required at NTS.
8. Consider possible mid-term program from standpoint of devices and possible sites.
9. Consider possible long-term program from standpoint of possible required facilities.
10. What can be done now to implement 8 and 9 above.
11. What impact, if any, does Gnome (readiness December 10) have on technical capability?

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The Board considered a number of specific items concerning NTS support facilities which would be the responsibility of Reeves' office. These included (a) improvements in housing, administrative laboratory space, and recreational facilities, and (b) augmentation of the labor force as soon as possible so as to establish a 40-hour workweek for the crafts and allow a 6- to 7-day workweek for specified areas when required to meet schedules.

After consideration of the mid-term program, shown in the second part of Table XVII, The Board recommended (a) immediate action to construct G tunnel for tests beginning in late spring or early summer 1962, (b) continuation of deep hole exploration for LASL high-yield events, including immediate engineering geology assistance and expansion of the long-range hydrological program (including possibly eight deep holes as part of the groundwater studies) to provide engineering data for high-yield test facilities, and (c) immediate initiation of studies and activities addressing the feasibility, site, costs, and schedules for a "Christmas Tree" facility, which both Livermore and LASL would share, should it prove feasible. It was further recommended that the NTS support organization be prepared to provide (a) mining crews to extend the U-12b complex, mine drifts, and shafts in the U-12 complex, to mine a new U-12g complex, to tunnel or drill a vertical shaft in dolomite, and to carry out postshot exploration in the Hardhat tunnel; (b) drilling crews to carry out Area 3 exploration and emplacement; (c) LASL deep hole exploration; (d) postshot exploration of Hardhat; (e) exploration of the Christmas Tree concept; (f) deep hole drilling for the hydrological program; and (g) drilling of many small diagnostic holes both horizontally and vertically.

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TABLE XVII
NTS PLANNING BOARD RECOMMENDED SCHEDULE OF EVENTS
OPERATION NOUGAT

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DOE, EXEMPTION 3

In addressing a possible long-term program and facilities to support it, the Board concluded that there was a valid and urgent need for an underground high-yield shot facility, that expanded support facilities for both technical and support personnel were required, and that a continuing program of three or more shots per month would be assumed for the indefinite future. The Board concluded that the Plowshare Gnome event would not have a major impact upon weapons testing except in the availability of technical personnel, a problem which could be resolved by adjustments in scheduling.

The group estimated that the total cost of the construction projects at the Test Site would be $6 million, the cost of those items recommended for immediate action being about $1,750,000.

The LASL diagnostics planned for the underground tests at this time (before the first tests) were alpha and yield measurements for both single- and two-stage devices, radiochemistry for single-stage devices, and time interval measurements for two-stage devices. Livermore, in a September 12 message from the Director, John Foster, to Betts, described plans to do alpha, high-explosive transit time, and pin measurements on the first test, as well as radiochemical yield determination. The types and descriptions of Livermore diagnostics planned for underground tests upon test resumption are listed as follows in a January 12, 1962, message from Sewell to Betts:

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5 U.S.C. 552 (b)(3)
DOE, Exemption 3

On the 13th and 14th of September, Ogle and the DASA staff discussed LASL radchem sampling needs for LASL shots. LASL wanted samplers available to monitor the underground shots but did not want to make a formal request since, of course, they were not supposed to vent any radioactivity. Some of these samplers (B-57s) were off in Australia and Colonel Kiley was worried about the rest of the planes being sent there, which might lead to unavailability of samplers in the event of U.S. atmospheric testing. Ogle noted in his diary that the B-57 people were willing and, in fact, anxious to fly a sampler on the LASL underground tests and that Kiley agreed to provide one for the first shot, after which future usage could be debated. Final considerations regarding firing Antler were discussed by the Antler Advisory Panel at the CP at 4 p.m. on the 14th of September. The Panel consisted of Al Graves, Chairman; Roger Batzel, Alternate Chairman; Clint Maupin, Orin Stopinski, O. Placak, Gordon Dunning, Gary Higgins, and P. W. Allen. Dale Nielsen presented the containment figures, and concluded that the probability of any measureable off-site fallout was extremely small. The Panel recommended to Jim Reeves, the Test Manager, that the device be detonated as scheduled.

Underground Testing Resumes

The Livermore Antler test of their [redacted] device occurred in location U-12e.03a at 10 a.m. PDT on September 15, 1961. Unfortunately, large-scale venting began at the tunnel portal several minutes after the event, leading to loss of virtually all the Livermore data. This development was to prove indicative of the problems with tunnel containment for some time.

General Betts sent a personal message to Hertford, Bradbury, Foster, and Schwartz on September 15 expressing his gratitude to each of their organizations for their response during the preparation period. He stated that, since August 31,

> DMA has had occasion to request a large quantity of information from all of you. Without exception, I can say that you have accepted my requests fully within the spirit in which given and your cooperation, both with respect to timing and with respect to substantive information, has been exceedingly gratifying. Also, you have been on top of the problem and have volunteered much information, in addition to the specific requests from DMA, which has been most helpful and beneficial. I know that this has meant a lot of work from, for each (Ed. note: from and for each . . .) of you individually, and for your organizations. I wish to take this opportunity to thank you for your help during this particularly trying period.

On September 16, LASL fired its first post-moratorium test, Shrew [redacted] in the U-3ac vertical hole, which LASL preferred over tunnel emplacement. Although it could have been performed as early as the 10th or 11th, as a result of political decisions noted earlier, it was the second test in the series, one day after the Livermore tunnel test.

Ogle reported on the first two underground tests at a LASL WWG meeting on the 19th of September. The Livermore Antler test [redacted] was fired at a point 1,760 feet below the surface. Alpha detectors were placed in the tunnel about 1,000 feet from the device. Not long after the detonation, radioactive debris began to escape from the tunnel entrance, from the ground above the device, and from a point above the end of one of the other tunnels. Later, a highly contaminated stream of water flowed from the tunnel entrance. The entire tunnel system, along with the alpha gear, had to be abandoned temporarily and recovery of the data was not expected. Speculation was that water, which was initially lying directly above the blast, drained into the blast cavity, vaporized, and was subsequently dispersed as radioactive steam. The LASL Shrew test [redacted] was fired in a vertical hole 330 feet deep which was backfilled with sand except for a sampling pipe leading to the radiochemical sampling pots. Containment was such that no particulate matter was found at the surface, but some air samples about 30 feet from the hole indicated seven mR per hour and one filter paper from the B-57 that did air sampling gave a count of about 20 mR per hour. Indications were that this activity resulted mostly from gaseous fission fragments.

Ogle also reported on the LASL schedule: [redacted] had been interchanged so that the latter test would be performed first in a vertical hole at the same depth as Shrew, [redacted] and with the same instrumentation. The present drilling capability would allow a shot approximately every other week. He reported that the water table was at 1,600 feet depth, which would place the [redacted] shot below water level, necessitating moving it to another location farther north. He also noted that arrangements had been made for core drilling in an area containing a granite bed extending horizontally for about six miles and suggested

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that the area offered promise as a site for very deep waterproof holes which might prove satisfactory for yields as large as one and a half megatons. [redacted]

Other WWG discussion led to a significantly modified schedule. [redacted] These would be held for possible atmospheric testing, presumably by airdrop over either the EPG or the ocean. Estimated delivery dates were six and eight weeks, respectively. At the next WWG meeting on September 27, Harold Agnew presented the newly-received, revised underground schedule for Nougat (Table XVIII), which reflected the LASL changes.

TABLE XVIII
REVISED NOUGAT SCHEDULE
September 27, 1961

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DOE, EXEMPTION 3

On September 22, Ogle sent to Reeves a brief description of the results of the first LASL underground event, Shrew. Quoting extensively from the summary:

> It was detonated at a depth underground of about 325 feet inside a buried canister 32.5 feet long. The canister was placed at the bottom of a 36-inch diameter hole lined with steel 1/2-inch thick. The hole was filled with sand above the canister to about 7 feet below ground surface at which point the filling became concrete. A radiochemical sampling system, basically consisting of 2-inch diameter pipe, came up from the device through the sand and turned to feed sampling pots on the surface whence it went into one of the old Hardtack shot holes used as a dump hole.
>
> The shot was detonated at 12:45 p.m., September 16, 1961. No immediate effects were noted; however, remote control radiation monitors indicated high radiation levels above ground. Early rad-safe surveys in the first half hour after the shot showed that practically all radiation was associated with the portion of the sampling system that is above ground. Radiation levels as high as 10,000 R per hour at a meter were reported as associated with one of the sampling collector pods. A B-57 sampler aircraft made four passes downwind from the hole and reported no activity. However, upon return to the field, it was found that the filter papers were at about 20 mR

per hour. At the same time, it was discovered that the filter papers in the air samplers placed some 30 feet from the hole on the surface of the ground read 7 mR per hour. The pulse-height analysis of the radiation from those filter papers showed that they had collected gaseous fission fragments such as xenon, krypton, or other elements with gaseous precursors. No statement can be made at the moment as to what proportion of the gaseous fission fragments reached the air, but data were obtained that will allow a later statement on the subject. No particulate radioactive material was noted on the surface of the ground even immediately adjacent to the hole. Thus, it seems proper to conclude that the particulate radioactive material from the detonation was completely contained; that there was some leakage of gaseous fission fragments, probably percolating right through the dirt; and that the radiation observed above ground around ground zero was almost entirely due to active material that was contained in the sampling system.

Since the sampling system was so hot, only small samples could be obtained from it. Laboratory analysis shows these were highly fractionated; however, it is expected that the sample to be obtained by taking all that is contained in one of the sampling pots will be moderately satisfactory. The samples will be recovered about September 23. Preliminary results from those samples indicate that the device went about as expected, but no precise yield can be quoted at this time. The alpha system operated properly and the results are close to those predicted.

I should like to comment that a great deal of credit is due to the AEC and its subcontractors, Reynolds and EG&G. The support we were furnished during the critical two weeks before this shot was outstanding and clearly made the detonation possible on that date.

Thus, after the first two underground tests of the new series in which each Laboratory was attempting complete containment of radioactivity, the technique using a tunnel drift in volcanic tuff had failed to achieve containment, destroying most of the data and rendering the tunnel complex unusable for some time. The technique using a vertical hole in alluvium had contained satisfactorily. Now, having achieved the politically desired early resumption of testing, the Laboratories could devote a little more attention to the real problems facing them, some of which were how to contain, how to get the best diagnostic information, how to improve drilling and tunneling, how to integrate Vela Uniform measurements with the ongoing test program, and how to perform high-yield tests underground. They might also investigate the problems of water table location and water contamination, development of the Christmas Tree concept, and possible acceleration of weapons development by atmospheric testing at the NTS, etc.

The specific objectives of the first two U.S. shots are of interest and are contained in a discussion of the AEC's General Advisory Committee on October 19 through 21 [redacted]

WITHHELD UNDER 5 U.S.C. 552(b)(3)

Operation Nougat

In a September 19 message, Bradbury informed General Betts that LASL felt it advisable to interchange the [redacted] The reason was the lack of experience with the containment depth-of-burial rules and a desire to gather more confidence by performing the lower yield [redacted] test first. The containment design for the [redacted] shot was thought to be very conservative at the planned depth [redacted] Furthermore, there would be a fairly trivial venting problem if the [redacted] shot should turn out to be inadequately contained.

On September 20 Betts notified the Laboratories and Operations Offices that verbal approval for Nougat had been received from the President and that confirming written approval would be provided to the field when such was received. Betts emphasized another point in this message; namely that the President:

> . . .is most, repeat most, anxious to accelerate time scale of this program. This expressed wish could stem from understandable impatience reacting to impact of very high rate of testing being carried out by U.S.S.R. However, interest in acceleration might also stem from concern that international, as well as local, pressures will build up over next few months and force cessation of testing. Accordingly, my previous instructions reference deliberate pace that could be sustained indefinitely must be modified to request that pace over next few months be maximum attainable in light of device, diagnostics, and site limitations. Accordingly, have your staff review program again to determine what steps can be taken to accelerate the schedule as now planned, perhaps introducing greater risk with respect to getting adequate results from each test.

Betts said he would meet with all of the addressees' representatives at Mercury on September 28 to thoroughly review the program and revise it as appropriate.

Norris Bradbury replied to Betts on September 21 concerning the possibility of accelerating the LASL program, describing what had been learned about underground testing to date, and suggesting how that foreshadowed the future of that technique. As for the only LASL test to date, he said:

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5 U.S.C. 552 (b) (3)
DOE, Exempion 3

Furthermore, on the present schedule, and particularly if the schedule were accelerated, "these samples are just going to pile up, any short-lived stuff that might have been helpful will disappear, and the problem will simply get worse. We are not arguing one way or the other, but hope people at high levels or any other levels are not kidding themselves about this aspect of life." In this same message Bradbury made some detailed arguments about quick-response atmospheric airdrops, noting that the Air Force ought to be able to drop [redacted] very quickly, within one week, and also setting forth the fact that Sandia and LASL were already "working like beavers" to get some (drop) cases [redacted] which could be done in about two months. One of Bradbury's final comments was that "the local quipsters are making the remark that the Russians will bury us in our own holes."

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5 U.S.C. 552 (b) (3)
DOE, Ex. 3

Expanded Testing Considered

In addition to underground testing, there were a number of parallel discussions going on about other testing possibilities, both self-initiated by certain agencies and in response to Washington requests. Very soon after the Russian test resumption the discussions about how to test more quickly in Nevada, as well as overseas, led to the question of atmospheric tests in Nevada. On September 13, Mr. S. P. Schwartz, President of Sandia Laboratories, advised General Betts that Sandia could provide tethered balloons at the Eniwetok Proving Ground and the Nevada Test Site. On a crash schedule Sandia could prepare an Eniwetok program to carry a 1,000-pound payload to 5,000 feet in about 90 days, and with the equipment stockpiled at the Nevada Test Site, could lift 2,000 pounds to about 1,500 feet within about three weeks. He also pointed out that LASL had developed a balloon-borne diagnostic Pinex capability during the moratorium. On the 21st of September Reeves informed the Labs that at the NTS there were four 4-foot shot cabs and one 6-foot cab, and Sandia was authorized to increase this inventory; there were six balloons that would carry 2,000-pound payloads to 1,500 feet, four balloons limited to 700 feet altitude, and five partial reels of cab control cable, limited in length to a maximum altitude of 1,000 feet unless spliced; helium trailers; and anemometers. Authorization was given to procure additional cab control cables and to rehabilitate the balloon areas in Frenchman Flat, Area 7, and Area 9. On the same day, Reeves asked LASL, Livermore, and ALOO to submit cost estimates by October 12 for various weapons test program segments, namely, (a) the possibility of developing a balloon contingency for both NTS and EPG, (b) work needed in the g tunnel complex, (c) Christmas Tree preliminary engineering, (d) site evaluation and geological exploration, (e) feasibility studies of (1) extension of main tunnel b, (2) exploration for deep holes for the LASL weapons program, and (3) the hydrological program; and (f) weapons and other related items for supporting each planned event. He also requested completion of event location plans for both Nougat and the so-called midterm program to begin after April 1962 (see Table XIX). Livermore, at this time, had used one tunnel location, which contaminated substantial parts of the tunnel, but they were projecting 10 more tunnel shots through the following August.

The September 21 Reeves memo on cost estimates was modified on September 30, as a result of the September 27 and 28 Planning Board meeting. The requested estimates for NTS work now included the following: shift of the event in dolomite to the midterm program; tunnel preparation for i, j, and k; preparation to emplace a 10-kt device in Area 3 as a possible added event; reentry and decontamination of underground facilities; coaxial cable procurement; a temporary power facility in Area 12; equipment; and assumption of Area 18 as the new location for the DOD Marshmallow event shown previously to be located in U-12e.01 or g. Regarding the balloon contingency program, Reeves noted that preparations were already under way for a contingency program in which five LASL events and one Livermore event would be switched to this method of execution, and he directed Sandia to develop an aerodynamic balloon capability at the NTS. EG&G was to furnish estimates for the following work: Area 7 alpha system; relocation of U-12e alpha system to i, j, and k tunnels; development of alpha systems for U-12e and U-12g; other activities addressing overseas testing; and expansion of the alpha detector inventory.

A September 25 message from Betts to Hertford requested a detailed proposal including a schedule and cost estimate for testing with balloons at either the NTS or EPG, but prohibited procurement of additional balloons or any flying of the balloons for the time being.

In one of the myriad of communiques between Bradbury and Washington on various possible atmospheric programs and justifications, etc., on September 25 Bradbury concluded another LASL shot list with the following quote: "After listening to highest authority's excellent speech before the United Nations this morning, we find it hard to see how all the foregoing can be other than an exercise for the student. Nevertheless, . . ."

The NTS Planning Board met on September 27 to prepare for a meeting with General Betts on the following day. Betts had asked the test community how the testing pace could be accelerated over the next few months to the maximum attainable test rate consistent with device, diagnostics, and site limitations. The Planning Board addressed three questions raised by Betts, namely, What acceleration is possible, utilizing existing underground sites? What acceleration is possible by resumption of

TABLE XIX
NOUGAT
September 21, 1961

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5 U.S.C. 552 (b)(3)
DOE, Exemption 3

Note: The Hardhat event, 5.0 kt, in U-15a, is included in both Vela Uniform and Weapons Test Programs.

Midterm Program
(All devices, yields, locations, and "R" dates very tentative.)

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5 U.S.C. 552 (b)(3)
DOE, Exemption 3

atmospheric testing at the NTS? What can be done in Pacific operations (airdrops, balloons, barges, etc.)? The Board reached the following very briefly stated answers for the first two questions on their first day of meeting. First, "The conclusion reached was that no acceleration was possible; further, that the present schedule was optimistic;" second, "The conclusion reached was that approximately five LASL events could be accelerated. The LRL events are device-limited, and balloon detonation would not accelerate the schedule." Later discussion about Pacific operations led to conclusions that two airdrops could be accomplished within two weeks; that a short-term air drop program with ground-based diagnostics could be ready in 2 1/2 to 3 months; that a continuing program would require six to nine months; and that a test of the Nike-Zeus [redacted] could be ready in six months at Johnston Island. In more detail, the Planning Board's discussions of the underground program focused first on preparations and limitations of various sites. The U-12e tunnel, which, prior to the Antler event, had been projected for seven events extending through the following June, was now contaminated. Decontamination work was proceeding at a rate of about 100 feet per day, consistent with a 3R exposure limit for the tunnel crews. The current exposure rate was 700-800 mR per day, thus limiting workers to about three days of work. Nevertheless, since U-12e provided the only high-yield site (up to 100 kt), the Board wanted all possible actions taken to restore it to operation. Steps were also being taken to add other sites for weapons tests. The i, j, and k tunnels, which were planned as safety shot tunnels, were now all to be extended, beginning in October and November, so as to have a capability for tests [redacted] The Board determined that the requirements for drill rigs must be reevaluated by the users. The DOD Marshmallow event which had been planned for U-12e.01 would have to be moved. The DASA representatives said that moving it to a new tunnel, U-12g, was unsatisfactory because of seismic interference from the weapons tests, the pipe alignment requiring about 4 1/2 months without disturbance from other shots. Thus, a new tunnel site would have to be selected and the DOD did not feel that a June 26, 1962, readiness date was feasible.

In addition to shot sites, cabling was a major problem, especially because of planned usage in U-12-i, -j, and -k tunnels and in the Gnome event. Furthermore, LASL, in requiring a new tunnel or deep hole for the [redacted] strained the cable requirements even more. The Board noted that Phelps-Dodge, the vendor at that time, was now running at maximum capacity, and foreign sources might be necessary.

Turning to atmospheric contingencies, the Board addressed the balloon capability, Sandia reporting an inventory of ten balloons, six with a capability of 5,000-pound payloads and four with a capability of 2,500-pound payloads, all to an altitude of 1,500 feet. They noted that the average expenditure on Hardtack was 1.5 balloons per shot. Areas 7 and 9 were now ready, they reported, but Frenchman Flat would require power, cable, and general overhaul. Cable was on hand or on order. Sandia said that they could support a shot within two weeks of authorization and if that were granted they could attain a seven-day readiness provided there was authorization for procurement of additional balloons (four- to six-week lead time) and for recruitment and training of balloon crews. LASL stated that they could have five devices ready on seven- to ten-day intervals for balloon testing. Livermore said that they were device-limited to a two-shot program and they preferred tunnels (if available) for the short-range program because of the diagnostics required.

The Board listed several EG&G actions for which EG&G had provided cost estimates; namely, establishing an alpha system for Area 7; moving the U-12e system to i, j, and k; and developing additional alpha systems for U-12e and U-12g.

The Board discussed and made several recommendations on capabilities needed at the Test Site for the Air Force, the Public Health Service, and the Weather Bureau. In these discussions, the use of towers for NTS atmospheric testing was not ruled out, but it was noted that balloons were preferred unless maximum diagnostics were required and adequate time were available.

The Board also discussed, at length, possible overseas atmospheric testing requirements, methods, and alternatives. Attached to the minutes were several detailed schedules: the one covering the Nevada underground and balloon programs is shown as Table XX.

The proposed LASL program of atmospheric tests was also discussed by Agnew at a meeting of the LASL Weapons Working Group on September 27. The proposal included balloon shots at the NTS, which would start immediately and continue with tests at 7- to 10-day intervals. Beginning with the [redacted] on October 1, the series would

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then include [redacted] At the same meeting, George Cowan reported on the results of the sampling pot technique used on Shrew. Of the four pots used, it was found that three did not throttle properly due to the high velocity of the sample. The gamma spectra of samples from the fourth pot looked normal, but processing was going slowly. Good results were expected, eventually.

TABLE XX
TENTATIVE SCHEDULE
NTS PLANNING BOARD MEETING
September 27, 1961

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5 U.S.C. 552 (b)(3)
DOE, Exemption 3

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Vela Uniform Reorientation

On September 15 DASA Headquarters directed Field Command to make whatever changes were needed so that current Vela objectives were achieved by getting data from the Antler, Eel, and Yukon events, and to gather close-in surface motion data from the Jordan, Pecos, and Chena events using funding allocated for Crystal.

Beginning on Nougat, the overall DOD management of the Vela Uniform measurements became the responsibility of the DASA Continental Test Organization (CTO). In parallel with these DOD changes, and as reported in the September 26, 1961, Planning Directive Number VU-9-9-61, the AEC Vela Uniform program was to be pursued, but, in particular, the DOD would be performing measurements on underground tests whenever possible. That directive set out briefly the technical projects including the following general areas:

> Earth motion measurements, such as particle motion studies and strong-motion seismic measurements, sponsored by DASA.
> Electromagnetic measurements, such as earth current measurements and surface EM measurements, sponsored by AFTAC.
> On-site inspection projects, such as vertical surveys of explosion sites, inspection of vegetation damage, and aftershock and subsidence measurements, sponsored by AFTAC.
> Seismic measurements, sponsored by AFTAC.

Some mix of these projects was pursued on all of the underground tests, beginning with Antler on September 15.

Nougat Continuation/Impact on Labs

Both Laboratory directors addressed correspondence to General Betts at the end of September, discussing the impact of the weapons test resumption on their Lab's programs and projecting supplemental funding needed to meet the anticipated future demands. Norris Bradbury, on September 29, wrote that from 80 to 100 additional people were needed in the weapons programs, with about half required from internal transfers immediately and the other half needed in the next 12 months. A very rough estimate of the increase in LASL weapons program costs would be about one and one-half million dollars in FY 1962 and two and one-half million dollars in FY 1963. Bradbury was attempting to meet all of the weapons program needs while not severely impacting the Rover and other reactor programs. From Livermore, Foster wrote to Betts on September 30, noting that in underground testing,

> In increasing our rate of progress, it is extremely important to increase the number of both the small- and large-yield underground facilities. Operational plans must assure sufficient facilities so that the temporary loss of any one of them will not materially slow the program. We are presently designing a high-yield "Christmas Tree" facility, and construction of an additional low-yield complex (G tunnel) has been started.

As for Livermore expansion, Foster requested a variety of items, including authority to expand four Livermore buildings, to increase the Laboratory manpower by 590 people by the end of Fiscal Year 1963, and to increase funding by 16% in FY 1962 and 44.6% for FY 1963 over the existing budget of $49.1 million.

LASL was projecting a possible firing schedule of two tests per month, as indicated by the proposed shot schedule circulated by Ogle on September 30. Following [redacted] (the Boomer event) on October 1, five events were scheduled: October 15, November 1 and 15, and December 1 and 15. A brief description of Boomer was

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given by Al Graves at the Boomer weather briefing on September 30. The hole used was U-3aa, approximately 340 feet deep, with the canister about 40 feet long. The hole was backfilled with sand to about 10 feet from the top and then filled with concrete, a configuration almost identical to Shrew. "The radiochemical pots on this device are set in concrete boxes which have 1-foot concrete lids. The pots and radchem parts are 6 feet underground and backfilled."

5 U.S.C. 552 (b)(3) DOE, Exemption 3

On the day that Boomer was detonated (October 1), General Betts reported to the Commission the success of the test, noting that there had been no venting. In view of this he inquired as to the advisability of announcing the test and the Commission agreed not to make a public announcement at that time. As it turned out, they never did.

General Betts reported at an MLC meeting on October 3 on the progress of Nougat, remarking in connection with the Antler contamination problems:

> They are now working back into the tunnel, washing down the walls, and mucking out the result.... Hope is that if they get past the Antler site, they will get into an uncontaminated area and to the other sites. One month delay in tunnel use is foreseen. Have put crews on other tunnels originally intended for safety tests. These tunnels may be raise [redacted] Are building up their drill capability. Now have 15 drill rigs, some of which are for radchem sampling but 5 of them are 36-inch affairs. LASL says they are not limited by drill capability.

Later in the meeting, in a discussion of the factors limiting various testing techniques, Betts reported that the three words limiting acceleration of the underground program were "cable, device, and site."

The magnitude of the cable procurement problem is illustrated by an October 5 memo from Jim Reeves discussing an October 2 meeting which concluded that contamination of facilities, delays in construction, and other factors could seriously affect the cable requirements. Reeves appointed W. R. Hickey of the AEC as chairman of a committee to review periodically this situation. The other representatives included Don Shuster of Sandia and Bob Newman of LASL. The three Test Group Directors (Ogle, Nielsen, and Kodis) were asked to collect their cable requirements for testing through the end of February 1962 and provide these to Hickey by October 15.

An October 6 message from Jane Hall, LASL Assistant Director, to Lieutenant Colonel Haney of DASA, briefly summarized the origin of the various components of LASL devices. All high-explosive components came from the Iowa Ordnance Plant at Burlington, Iowa, with minor exceptions. All firing sets were provided by LASL Group GMX-7, again with minor exceptions. All zippers were provided by Sandia, zero racks for the devices were provided by LASL/EG&G, and the canisters were prepared by LASL. Final assembly of the pit in the HE was performed at LASL. All gas reservoirs were obtained from Savannah River Plant (SRP) and the nuclear materials were obtained from Rocky Flats, LASL, and Oak Ridge (Y-12).

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On October 7, AEC Chairman Seaborg, in a letter to President Kennedy, noted that the underground test program must be supplemented by atmospheric testing if nuclear testing was to be accelerated and increased in scope.

As of October 9, while the U.S. had detonated three very small devices underground, the Russians had fired [redacted] devices in several different locations with a yield totaling [redacted] On that day Deputy Secretary of Defense Gilpatric wrote a letter to the President which included new recommendations for atmospheric and other types of testing and stated:

> Though a limited amount of valuable data can be secured from the current underground test series with low-yield devices, it is being obtained at a relatively slow pace as is characteristic of underground testing. It may also be emphasized that testing underground can neither provide all the effects data necessary to satisfy defense

> requirements outlined above nor permit the most rapid and full exploration of high-yield weapon technology by the Atomic Energy Commission Laboratories. If it is desired to accelerate the present schedule of operations at the Nevada Test Site significantly, it will be necessary to use balloon techniques.

He then set out several specific device tests scheduled for underground detonation and suggested how they could be accelerated by one or two or more months, given a balloon technique. Finally, he recommended that the DOD and AEC be authorized to prepare for atmospheric testing at the NTS, among other locations.

On October 10, in a letter with a similar goal from Chairman Seaborg to President Kennedy, Seaborg referred to the earlier recommendations for preparation for low-yield atmospheric tests in Nevada conveyed jointly by Secretary Gilpatric and Seaborg on September 20. He noted that the quickest way to accelerate the U.S. test program would be to do some of the Nougat tests one to two months ahead of the current schedule by:

> Using tethered balloons at the Nevada Test.... Balloons are on hand. Rehabilitation of the ground handling equipment and training of the crews are the pacing factors of the steps required to achieve readiness; these steps will take from 10 days to 2 weeks. Sampling aircraft and diagnostic equipment can be ready within the same time frame. An advance of as much as 4 or 5 months in the schedule for some of the tests in the follow-on program enclosed with my letter of September 19, 1961, could also be accomplished by use of the balloon technique. Toward the latter part of this program, testing could be conducted much more rapidly if not inhibited by the lengthy process of preparing underground sites. The cost of balloon tests is appreciably less than of those conducted underground, or those utilizing towers, but there is some sacrifice in the technical information attainable.

On October 10 DASA informed George Bing, who was then working on Vela Uniform with ARPA, of the tunnel problems at the NTS, noting that the contamination levels within the U-12e tunnel now promised to "forestall any further events in U-12e for several months. Consequently, to the best of our knowledge, the AEC is planning to open up four additional tunnels tentatively known as g, i, j, and k." While this information was part of a discussion of the Vela Uniform participation in NTS testing, it gave new information on the delay caused by the contamination from the Antler event.

The second Livermore test (and the fourth in Nougat), Chena, was detonated on October 10 at 10:00 a.m. in tunnel U-12b.09. Thirty minutes after the event it was reported from Mercury that containment had been achieved inasmuch as radiation stations in the main tunnel, at the portal, in the near vicinity of the portal, and on the mesa were reading background radiation only. However, only a few minutes later radioactive gas started coming from the stack (directly above the zero room), indicating that the cavity had collapsed, and at one hour past the detonation the reading at that location only was 2 R per hour. Preliminary indications from remote tunnel indicators were that damage in the tunnel was confined to the b.09 drift, although this was only shortly after the shot and there had, of course, been no thorough examination of damage. Reentry of the tunnel was to commence the next day. A few hours later, according to the H-plus 6-hour advisory, the radiation detectors in an area extending 200 feet from the tunnel portal indicated an exposure rate of 10 R per hour. Preliminary indications were that the yield was within the expected range. Ultimately four working days were needed for decontamination and rehabilitation prior to any further construction in the b tunnel.

An October 12 memorandum to Al Graves from the radiochemistry group reported briefly on the results of radiochemical sampling and yield determination on the first two LASL underground events. Both events had exhibited extensive fractionation in pipe samples, and while the phenomenon was not a surprise, "It is greater than had been expected." Analysis based on the samples available had led to a wide variation of yield, depending upon what isotope was considered in the arithmetic. Various arguments were made to decrease the variance of the calculated yield, but as yet there was still confusion and the results were inconsistent. The radiochemists hoped that the core samples obtained from drillback would yield more reasonable results, as had been the case in previous experience, but the desired core samples were not yet in sight. In summary, they noted: "The chief point in this memorandum is that our experience with prompt sampling for radiochemistry on underground tests has, to date, been unfavorable."

The Boomer rad-chem results were also discussed at the October 12 LASL WWG meeting. It was suggested that the sample pipe probably closed early, giving rise to samples of much different appearance than those obtained from Shrew. In addition, the Boomer sample pots did not contain liquids as was the case with Shrew. However, the Boomer samples were even more highly fractionated. For both events the blower system on the surface collected activity due only to rare gases and their daughters. This observation was interpreted to imply that on the Shrew test the blower system was sampling krypton and xenon that had come up through the sand, but that on Boomer all blowers received the same sample coming largely from the dump hole.

In an October 12 letter from Newman and Ogle to Reeves, LASL proposed to find a more suitable shot area for deep tests than the NTS. Two types of sites were suggested as meeting the requirement for the firing of large devices without introducing contamination into a usable aquifer; namely; (1) an area which contains no underground water or (2) an area in which the underlying soil or rock is so impermeable as to preclude movement of contaminated water into a usable aquifer. LASL would have preferred the entirely dry area and suggested some guidelines as to what areas might be studied. What they were requesting was a search of the literature followed by appropriate reports which would include maps of interest with subsurface contour maps and drawings of area lithology.

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5 U.S.C. 552 (b) (1), EXEMPTION 1
DOE

[redacted] 5 U.S.C. 552(b)(3)
Ex. 3, D.O.E.

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5 U.S.C. 552 (b)(3)
5 U.S.C. 552 (b)(1)
DOE, Exemptions 1 & 3

Further Consideration of Expanded Testing

A message from Reeves to the NTS Planning Board on October 13 noted an October 18 meeting to discuss the DOD Hardhat event and requested that the Board also be prepared to provide criteria and justification for resumption of atmospheric testing at the NTS utilizing balloons. Reeves wanted a list of devices to be tested, a recommended schedule, justification for the method, requirements for helium, and assessment of the impact of this method on NTS support facilities. Further, Field Command was asked to submit detailed plans and justifications for effects tests that would require atmospheric detonation.

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*See Chapter IV.

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At the October 12 LASL Weapons Working Group meeting, Ogle summarized the projected LASL testing schedule and budget (including the next fiscal year) which had been submitted to DMA. [redacted]

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Consideration of the Christmas Tree concept included a study by the corporation run by Roland Beers, known as RFB, Inc., which was evaluating the concept as requested by Jim Reeves early in October. On October 16, after studying the concept to some degree and looking at some of the data from the early underground shots, RFB people were in the midst of evaluating the separation of shot points and the various laws to be followed in setting out the configuration for safety, containment, lack of contamination, access, and other considerations. Generally, they felt that the 600 $W^{1/3}$ scaling law for the separation of shot points from the access shaft and the 50 $W^{1/3}$ law for the spacing of the shots around the ring were possibly inadequate. While noting that there would soon be a better understanding of this from current data, they suggested providing a second access shaft, extending the working radius further out from the shaft and providing greater spacing between each shot around the ring. Two days later, Mr. Collins of Holmes & Narver submitted his company's preliminary drawings illustrating the Christmas Tree configuration as well as views of the camp and tunnel facilities.

By the middle of October, with four underground tests behind them, the Laboratories had changed very little their feelings about the suitability of the underground regime for effective weapons testing. At a lengthy Commission meeting on October 17, with the Laboratory Directors and Managers of the Albuquerque and San Francisco Operations Offices present, Bradbury expressed his view that it was extremely difficult to acquire reliable data in the underground medium, which some at LASL had always held to be the case. He noted that in one test the measurements had been impaired by the underground location and the close proximity of material interfering with the alpha measurements; also, the radiochemical results had been marginal since the samples were badly fractionated. In response to a question from Commissioner Wilson, he observed that, as yet, seismic measurements were not satisfactory for determining the yield and that there was insufficient experience in seismology of weapons detonations in heterogeneous media. Foster briefly summarized the results of the two Livermore tests: only an approximate yield determination had been possible for the [redacted] since no alpha measurement was obtained and only limited radiochemical samples had been acquired after the test. [redacted]

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Foster expressed his general feeling that, although some smaller weapons could be more effectively tested underground, he preferred above ground testing for the most part since it resulted in better diagnostics. Bradbury agreed that in almost all cases it was desirable to test above ground. The subject of tunnel contamination was then raised by the Chairman, and Foster said that the Chena tunnel contained radioactive material immediately following the explosion. Blowers turned on the day after the test forced the contamination out of the tunnel, and only several days later was the tunnel reentered to find that it had been blocked by debris at the juncture with the connecting tunnel. Foster emphasized, as he had quite recently, that adequate tunnel space was not as critical a pacing factor for Livermore at this time as the

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lack of sufficient test devices. The latter shortage was primarily a result of the concern that an acceleration of activities during the moratorium might have touched off adverse public reaction. He also reported that during the moratorium, the psychological reaction to not being able to test had permeated the Laboratory and adversely affected its productivity. Furthermore, even if the U.S. decided to resume atmospheric testing, Foster stated that tunnel construction should be accelerated. Bradbury continued, still very uncertain of the underground method, that he would prefer to retest in the atmosphere, if the U.S. decided to resume such testing, those devices which had been tested only underground. After Commissioner Graham recalled the 1958 attitudes of Livermore and LASL about underground testing as being pro and con, respectively, Bradbury said he still believed that the merits of underground testing were overestimated, although such testing was better than none at all. Foster emphasized that if the U.S. continued underground testing exclusively, it would require a considerably greater level of effort over a longer period of time to acquire optimum diagnostics. Thus, he felt the U.S. should resume atmospheric testing, but at the same time underground test preparations should be accelerated in order to acquire the technical capability to test in that regime and to best allow for the possibility that political developments would again require testing underground exclusively. He suggested that the AEC spend an additional $50-$100 million to advance tunnel construction. Bradbury said that while he did not disagree with arguments favoring increasing development of underground testing, he was hesitant to support a $50-$100 million increase. Edwin McMillan, Director of Lawrence Radiation Laboratory at Berkeley, who entered into the discussion at this point, endorsed vigorous underground testing and tunnel construction, but said that the U.S. should resume atmospheric testing since he believed underground testing would never be an adequate substitute. Schwartz of Sandia endorsed the others' feelings that underground testing was better than none at all, but that atmospheric testing was more desirable since efficient techniques in that regime were already developed. In response to a question from the Chairman about where underground capabilities should be developed, Foster felt that NTS would be adequate for a period of a couple of years, but suggested that other sites should be ready after that. He estimated that Livermore would have developed their technology in about five or six months to the stage where they could test two devices per month, and, including LASL testing at a similar rate, this would mean about one test per week. Foster said he would prefer to continue that rate even if the U.S. decided to resume atmospheric testing. A discussion of accelerating the underground program then included Hertford of ALOO, who commented on the adequacy of current tunnel construction, noting that he had the crews now working continuously (24-hour days, 7-day weeks) on i, j, and k tunnels. With additional funds, ALOO could have more tunnels constructed. The Chairman agreed that acceleration of the underground program was necessary, but that this must be balanced against funding required and examined in the light of other national needs. Commissioner Wilson expressed his opinions about underground testing techniques in a continuation of the meeting with the Laboratories and Operations Offices later the same day, based on information from Hertford that because of repeated weapons tests, fissures had developed in the ground above the tunnels. Consequently, Wilson suggested that testing in vertical shafts, as LASL had done, might be more useful than tunnel testing. Hertford then argued (incorrectly, in retrospect) that vertical-hole-testing presented many of the problems common to tunnel testing, such as steam venting and possible contamination of groundwater. In lengthy discussions of the various Laboratory weapons development programs that had either been accelerated or deemphasized due to the press of getting devices ready for testing, Foster and the others in the afternoon discussions addressed just what the underground program objectives were at the moment. [redacted]

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[redacted] and Foster stated that the significance of any results from the current series for the Hardtack weapons development program would not be fully realized for some eight months after the tests. Chairman Seaborg asked that a study be prepared on what the scope of underground testing should be in the event the U.S. resumed atmospheric testing. Wilson noted Foster's earlier recommendation for an additional $50-$100 million to advance tunnel construction and said that he rather agreed with LASL's approach to testing in vertical shafts, "noting that several of the tunnels had been seriously damaged by detonations and existing tunnel space will permit only limited testing." Commissioner Haworth then argued that when the tunnels were built, the pressing time schedule had limited the quality of the tunnels. The AEC General Manager, Alvin Luedecke, went even further in recommending that if it could be demonstrated that 2,000-foot shafts could be properly instrumented, he would recommend abandoning the tunnel approach. To him it was evident that there was no full containment except at extreme depths and that even the slightest degree of venting would both arouse public concern and in fact raise the radioactivity level. Thus, weather conditions at the time of underground tests must be taken into consideration. Commissioner Haworth then suggested that tunnels could best be used for small-yield tests where only slight fracturing would occur. Luedecke, however, pointed out that even in the smaller tests, serious fracturing had taken place. The Commission concluded its meeting by requesting recommendations on the scope of the underground capability, taking into consideration the comparative advantages of tunnel and vertical hole shots.

On October 19 Bob Campbell, Assistant LASL J-Division Leader, sent Jim Reeves a letter responding to Reeves' September 21 request for cost estimates from the Laboratories and others. Campbell set out several LASL projections for the general scope of testing over the next 20 months (through the end of FY 1963). [redacted] The heavily instrumented tests would be done in shafts* with short, heavily instrumented drifts at the bottom, rather than in holes, and would require personnel access to the zero point, collimated lines of sight for instrumentation, and complex sampling equipment, among other things. [redacted] It should be stressed that LASL's attitude at this point was that if atmospheric testing as desired were possible, the underground test method would be abandoned during that period. Based on these numbers of projected tests, Campbell set out specific requirements to support the scenarios as follows:

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1. **Alpha Systems:** Three systems of 60 oscilloscopes each were required in addition to what was then on hand: these were to be obtained, installed, and operated by EG&G under LASL technical direction.
2. **Detectors:** EG&G was to furnish alpha detectors as needed to support the test scenarios.
3. **Portable Alpha Station:** H&N and REECo were currently designing and constructing such a station for use at NTS and this should be completed.
4. **Coaxial Cable:** These requirements for underground tests might be scaled directly from the Nougat requirements.
5. **Update Balloon Area:** LASL wanted an alpha station of sufficient size to hold a modern alpha recording system and, if possible, a winch and detector arrangement to permit using more than one ground-zero.

*A shaft is a mined vertical hole with drifts at the bottom. Holes are drilled.

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6. **Timing and Firing:** Independent timing and firing systems for Areas 3 and 7 at the NTS were desired.
7. **Zero Racks:** A zero rack for each shot would be furnished by EG&G.
8. **Zippers:** Zippers would be required from Sandia for most shots.
9. **HRT (High-Resolution Telemetry):** Sandia's assistance with this system would be required on all above ground shots.
10. **Underground Hydrodynamic Yield:*** Sandia, in conjunction with J-15 at LASL, would continue their attempts at obtaining underground yields by hydrodynamic methods.
11. **Assembly Facilities:** A weapons assembly facility physically separated from the existing complex was strongly desired to preclude the unnecessary safety and scheduling problems due to the present arrangement of sharing assembly and storage facilities at the NTS with other organizations.
12. **USGS:** The USGS effort at NTS should continue as necessary to support underground testing.
13. **Weather:** LASL recommended that the present facilities be augmented by upper wind stations at Shoshone, Alamo, Tonopah, Beatty, and Indian Springs for any atmospheric NTS tests.
14. **Aircraft:** Sampling aircraft requirements as detailed elsewhere.
15. **Shop Facilities:** Present NTS facilities were adequate for this.
16. **Radchem Lab:** A Radchem Lab would be required at the NTS for early yield determinations should conditions of firing such as in Hardtack Phase II prevail again.
17. **ECM:** In the event of atmospheric testing, ECM or comparable equipment would be required to locate sources of electronic interference.
18. **Housing, Transportation, and Communication Requirements:** As on previous operations where required, it was hoped, because of the continuing nature of these programs, that single occupancy quarters would be made available.

In its meeting of October 19-21 the AEC's General Advisory Committee heard detailed briefings and had lengthy discussions on the test resumption, the various techniques, and the possibilities for weapons testing in the immediate future. One of the briefings presented showed a chart giving the relative advantages of the different types of testing, such as what sort of diagnostic data could be gathered. This chart is shown in Table XXI. In light of the poor results thus far in radiochemistry work and the efforts being put into this area, Commissioner Wilson noted that such work had fallen about six months behind schedule and that the Commissioners were desirous of employing additional good radiochemists. Willard Libby, a former Commissioner, asked if the Commission had canvassed Argonne and Brookhaven to see if some of their radiochemists who might not be willing to leave their laboratories permanently would be willing to receive some of the samples for analysis there and thus alleviate the work load. Libby generally advised that since underground testing was in the development stage, improvements in technique and diagnostics could be expected, to which General Betts agreed. In a discussion of the overall status of the Test Site activities, Captain Craig of the DMA test office explained that Livermore was expanding its tunnel complexes at Nevada and there were 12 to 16 drill rigs drilling holes for LASL devices. (See Figure 10.) He briefly described the Livermore

*A September 7 entry from the author's diary describes a concept for making hydrodynamic yield measurements on underground shots. The proposal was to drill a small-diameter (perhaps 8-inch) hole alongside the main hole but about 10 feet away, and another smaller (perhaps three-inch diameter) hole within the next 30 feet (Ed. note: along the same radius). Time-of-arrival and pressure-versus-time measurements made at the bottoms of these holes would allow J-15 to make yield estimates, it was felt. Potential problems were largely in the capability to determine the distances from the emplaced device to the bottoms of these holes and in the time and effort to drill the holes. The first shot for which these could be performed would be in the middle part of November, but unless accurate position measurements could be made it was not clear that there was any point in this technique. The decision was made to have J-6 investigate these problems and have J-15 think a little more about yield determination from measurements made within the canister itself or in the main hole.

TABLE XXI
COMPARISON OF DIAGNOSTIC QUALITY

Diagnostics	Underground vs Atmospheric Testing	
A. Prenuclear		
1. Detonator signal	good results	good results
2. HE burn signal	good results	good results
B. Nuclear		
1. Fission (n, gamma)	good results	good results
2. Fusion (n, 2n)	good results	good results
C. Postnuclear		
1. Fireball growth	no results[a]	good results
2. Bhangmeter	no results	good results
3. Shocks	poor results[a]	good results
4. Radiochemistry	poor results[b]	good results

[a] Clearly not possible in underground tests because of limitations arising from fireball and nuclear shock interactions with the cavity. Shocks and blast waves in the surrounding earth can easily be detected, but inferences about the details of the nuclear source are highly uncertain.

[b] In 1968, ff, these technologies were completely in hand and underground testing had demonstrated its flexibility and superiority for all but atmospheric effects purposes.

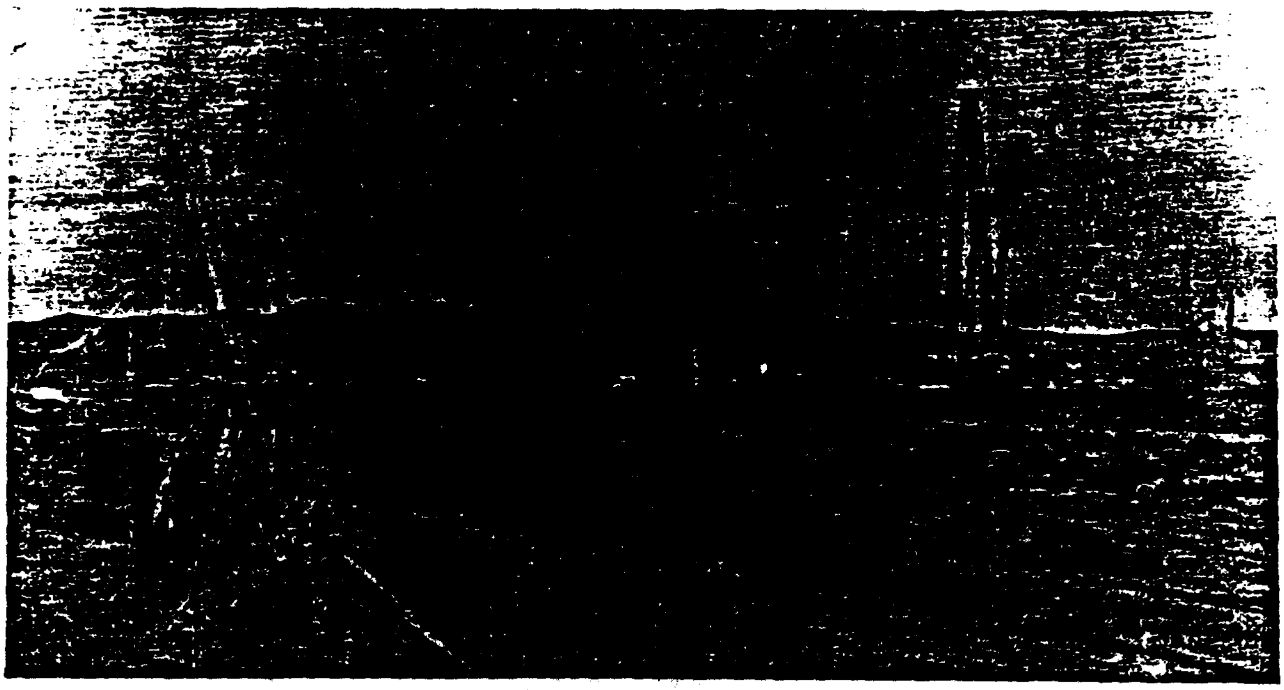

Figure 10.
A twilight photograph showing some of the drill rigs.

Christmas Tree tunnel concept as having about 15 shot locations, the deepest at about 4,000 feet, which could accommodate yields as large as 200 kt. After a couple of days of discussions, which included detailed looks at the possible testing scenarios for the future, Libby recommended that, since the U.S. might not be able to test in the atmosphere, the underground techniques should be emphasized, and he mentioned the possible establishment of an additional test site in a salt dome such as the Mississippi-Louisiana area, where exploratory drilling operations were currently under way in preparation for site-selection for the salt-medium decoupling experiments in the Vela Uniform program. Abelson, another GAC member, while noting that there were some aspects of underground testing that actually presented very considerable advantages, also pointed out that radiochemical samples could be collected about as easily underground as in the atmosphere. Since the committee seemed to agree on these points, the Chairman, Ken Pitzer, asked Libby to draft the General Advisory Committee's view on weapons matters for their recommendations. Thus, the October 21 letter to Chairman Seaborg with the recommendations of the General Advisory Committee contained the following under the heading "Underground Testing":

> **The committee believes strongly that the Commission should continue urgent development of the underground testing technique, even if atmospheric testing is resumed. In particular, the possible use of the salt domes in Mississippi and elsewhere for underground testing of devices of larger yield than can be handled in Nevada should be thoroughly explored. The forthcoming Gnome Plowshare shot should give valuable information about the usefulness of the salt medium. Other underground formations as well as outer space should remain under consideration as sites for testing.**

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Nougat Continues with Revisions

Jim Reeves sent a message to the Labs on October 20 discussing the DOD Marshmallow event, citing the Planning Board's investigation of possible sites, and identifying U-16a in the Shoshone Mountains as a possible location. While this [redacted] underground test was to be emplaced, stemmed, armed, fired, and instrumented by the AEC, the majority of the information to be obtained was for the DOD. Reeves asked for the addressees' comments on a construction start date of about November 1.

The same day, Bob Miller remarked to Jim Reeves on a number of items, including the DOD Nevada tests. At the DOD's request Sandia was to handle all of the AEC technical participation on the Hardhat event, including Vela Uniform (Lollipop) and Plowshare measurements by Livermore, as well as arming, timing, and firing. The DOD had requested the same relationship for the Marshmallow event, for which, Miller said, the Labs agree with the DOD's choice of a Tippipah Springs site. (See Figure 11.)

LASL's concern over the lack of good diagnostic data in underground testing was emphasized in an October 24 message from Bob Newman to Joe Sanders stressing the necessity for LASL to get radiochemical samples within two weeks after a shot in order that they be relatively unfractionated. Any greater time for recovery resulted in great uncertainties, and Newman said, "It is doubtful we can continue to operate underground unless we can get samples within two weeks after the shot." Thus, more pressure was put on the field test organization to improve their drilling capabilities. The future work load in that particular area would certainly be heavy, based on an October 25 message from Betts to the Labs. Betts' list, which had been submitted to the President for approval, [redacted] extending from March through September 1962.

On October 23 Ogle reported to DMA on some problems in preparing for the Mink event. Mink was originally scheduled for October 1 in an 800-foot hole, which was to

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SECRET

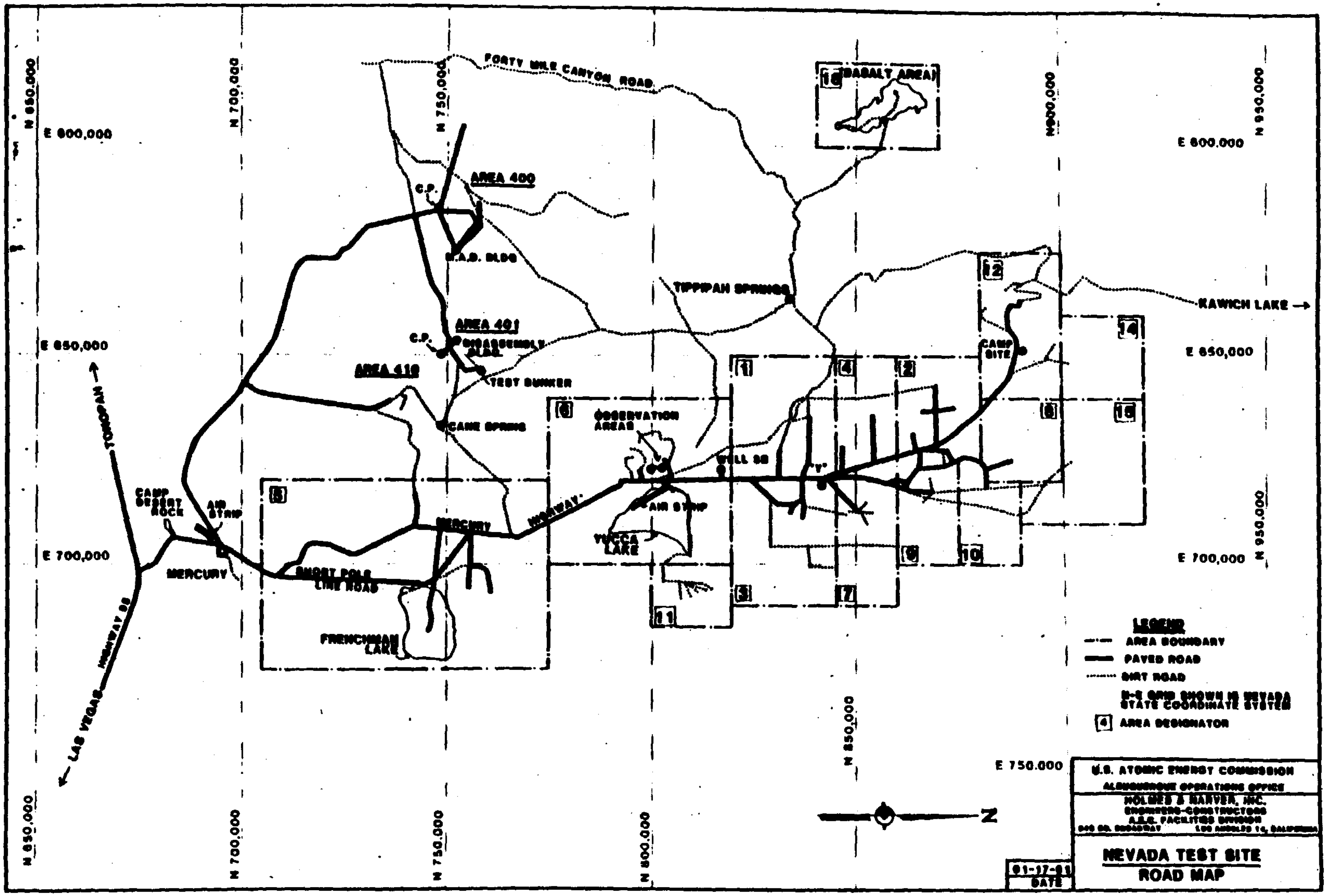

Figure 11.
Early 1961 map of NTS.

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be obtained by deepening the existing U-3ae 500-foot hole, but difficulties with lost tools and other problems resulted in delaying it and putting Boomer in that hole. "It was a bad guess on my part to try to deepen the 500-foot hole. U-3ae is now completed at 640 feet, and preparations for use are moving rapidly, i.e., putting in coax, sampling system, etc." Although it was hoped to fire on October 28, this could easily slip to October 29. Dormouse, [redacted] was originally scheduled to be fired in U-3ah at 1,200 feet on November 1. Due to the addition of small holes drilled for attempted hydrodynamic yield measurement, which would not be finished until October 29, LASL was now hoping to lower the device on November 3 or 4 for a firing date of November 5 or 6. The main hole was now at 1,200 feet and casing was under way. Packrat, the next event scheduled, was originally to be November 15 in U-3ai at 1,200 feet. That hole was now at that depth awaiting casing, which could not be done until after casing U-3ah was completed because of lack of tools. After that, LASL expected to be able to do the shot one week after Dormouse, or about November 12. Thus, Ogle felt that, overall, LASL hoped to pick up a little time on the original schedule.

On October 25 the status of DASA funding needs for the various testing possibilities was forwarded to the Secretary of Defense. Of the total $40.5 million increment required for various projected test possibilities, $2 million was for underground test participation in FY 1962 and an additional $4 million would be needed to complete the Marshmallow test.

At LASL, the radiochemistry group (J-11) reported on October 25 that drilling for the core samples of the Boomer debris had not yet begun since the appropriate equipment was still engaged in trying to obtain core samples from Shrew, tested on September 16.

J-6 of LASL also reported on the status of the construction work at the Test Site on October 25. During the month, drilling and casing to extend U-3ae from 500 feet to 640 feet deep had been completed and casing had begun on U-3ah and U-3ai, indicating that drilling of these 1,200-foot holes had been completed. One other hole (U-3ak) had been dug to 1,215 feet and another (U-3am) was in progress, along with a number of smaller-diameter holes for diagnostics, dump holes, and exploration. Two new postshot drill setups had been ordered and were due for delivery by about November 24. A 100-ton crane was also ordered. H&N and REECo had been asked to propose a dry drilling method that would work in Area 3 for postshot and instrumentation holes. Coaxial cable for Area 7 balloons and J-15/Sandia time-of-arrival measurements had been ordered. J-6 had also spent some time with Sandia and DASA discussing stemming, radiochemical sampling, and operational problems with the Hardhat event, and had participated in discussions between Livermore, H&N, and the AEC on the Christmas Tree concept. J-8 of LASL, also reporting on October 25, had supported the Boomer event with pressure measurements and now had sufficient equipment for three pressure channels on Mink and on future events. They also had worked on planning for supporting timing and firing for 1962, in coordination with Sandia and EG&G.

An October 25 message from Hickey of ALOO noted that coaxial cable delivery schedules for the Nougat events had been worked out up through the end of 1961. It had been confirmed with Phelps-Dodge, the coax supplier, that additional cable could not be delivered earlier than certain specified dates in January, February, and, in some cases, April of 1962. Since the requirements for Nougat events after January 1, as well as the two DOD events in 1962, had not been provided to the cable supplier and answered with appropriate delivery schedules, there was a possibility of not being able to meet some of the shot requirements. Hickey also noted that the users had not yet supplied their requirements for the mid-term program (after February 1962). The next day Jim Reeves asked the Test Group Directors of LASL, Livermore, DASA, and Sandia to review their scheduled Nougat event requirements based on the 1962 delivery dates for cable provided by Hickey. He further asked what relief would be gained if atmospheric testing were authorized and whether there were acceptable substitutes for any of the coaxial cable, "thus allowing program to continue after January 1."

An October 27 meeting of the LASL Weapons Working Group discussed the interesting fact that the Dormouse site was being delayed by difficulties in casing, possibly for several weeks. At the same meeting it was stated that balloon shots, if authorized for the NTS, would begin after the Mink shot, with a repeat of the Shrew device to verify the yield, followed by Dormouse and, tentatively, the rest of the Nougat schedule as approved. As a contingency, a pit for a repeat of the Shrew event had been ordered and was scheduled to be available at the Test Site on November 6.

General Betts wrote to the Laboratories on October 27, asking for several detailed comparisons. He wished to know the advantages, if there were any, of underground testing, a comparison in cost and time and instrumentation limitations for the vertical hole and tunnel techniques, and what should be pursued in the way of underground facility preparations in the event that atmospheric testing was authorized. He wished the Labs to answer by November 3.

On the same day, the Commission's General Manager, Alvin Luedecke, wrote to the Joint Committee on Atomic Energy summarizing the underground tests to date and including a lot of detail on current planning for future testing, underground and in other environments as well. Among other things he stated that approval for balloon testing at the NTS would permit acceleration of certain underground tests scheduled in 1962 by as much as four or five months.

On October 28 Norris Bradbury gave Betts a list of the accelerated execution dates of various planned Nougat tests if balloon testing at the NTS were permitted. He stressed that not only would the tests be accelerated, but that more accurate and more easily executed diagnostics would be possible. Kenner Hertford replied for ALOO on the same day, noting that ALOO could reduce the response time for the balloon program by two weeks if given the authority to fly practice balloons for training purposes. John Foster of Livermore replied the next day with several complete test series possibilities, giving the various options. His schedules all showed underground testing continuing even if atmospheric testing were authorized.

In an October 30 letter from Chairman Seaborg to President Kennedy, Seaborg stated that whether we did atmospheric testing or not, it was mandatory that the underground program be continued, very much in line with the Livermore opinion and not in line with Bradbury's approach to move completely above ground for the time being if that were possible. In a meeting three days later the National Security Council recommended that underground testing continue in parallel with preparations for atmospheric testing.

After delays in the emplacement process caused slippage of one day, Mink was detonated on October 29, and the initial report said that there was a small flash and some small gas seepage. Several hours later significant radiation levels were confined to areas within a 25-foot radius around each sample pot and the dump hole, and there was no radiation off site. There was one unusual incident: the Air Force B-57 sampling aircraft carrying Paul Guthals, the sampling controller for the tests, struck a weather tower on its second pass over ground zero, lost a sampling tank, and sustained some damage to one wing, requiring it to land nearby at Indian Springs Air Force Base. The tower crumpled and was a complete loss.

Communication between DMA and LASL at the end of October indicated the status of diagnostics on underground tests and the dearth of information from the underground techniques. General Betts noted that they had not yet received a firm yield figure for the Boomer event conducted at the beginning of October. Bill Ogle answered that

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the only figure LASL could give at the moment would come from the prompt radiochemical samples. [redacted] The alpha measurements were about as predicted, but these did not give a precise yield figure. The various hydrodynamic measurements as yet had not provided yield figures. Thus, a good yield measurement was expected to come only from the core radchem samples after drillback, but that had not yet been completed and might take some time.

A message from Batzel to Betts on October 31 concerned the planned underground shot in dolomite. The fact that it was to be performed in a medium not yet used made it very important to Livermore that the device detonated be one whose yield was already well known. In addition, Livermore urged that radiochemistry not be the method relied on for yield determination due to the uncertainties of fractionation behavior in dolomite. Batzel also pointed out that because of the press of other underground construction for more immediate test requirements, Livermore was delaying the conduct of this shot.

These and other changes were the beginning of a shift in Livermore underground testing philosophy, no doubt based on their negative experiences with containment and contamination on their first two tunnel events. Milton Rex of the AEC wrote to Betts on October 26 about what had been discovered on reentry into both of the main Livermore tunnels, U-12b and U-12e. He described the areas that were free of radioactivity and the amount of debris blocking various areas, and stated that:

> The Laboratory (Livermore) plans to continue the current operations plan until the conditions beyond the 12e.03 intersection are known. At that time, a determination will be made as to the practicality or possibility of further operations in the 12e complex. It should be noted that reclamation activity is proceeding at a faster rate than would be possible for excavation of a new tunnel. It is hoped that radiation levels beyond the intersection will decrease and that reclamation progress will improve. We cannot predict at this time the impact of the reentry schedule on the present event schedule.

Livermore once again explicitly made known their feelings about development of the underground testing technique in a message from Batzel to Betts on October 31. Batzel began by noting that, to Livermore, the underground shot program was essentially independent of a decision to return to atmospheric testing, particularly with respect to preparing sites. He then presented a revised schedule, prefacing this with "Based on the LASL experience in Area 3 and the improved situation in e tunnel, we have developed a new site plan to match the present schedule." This schedule is shown as Table XXII.

TABLE XXII
LIVERMORE NOUGAT SCHEDULE
October 31, 1961

Event	Date	Shot Site
Mad	11/30/61	U-9a
Feather	12/15/61	U-12b.08
Platte	01/15/62	U-12k.01
Brazos	01/20/62	U-12e.04
Rogue	02/01/62	U-9b
Kuskokwim	02/05/62	U-9c
Cheyenne	02/10/62	U-9d
Jordan	02/15/62	U-12i.01
Columbia	02/20/62	U-12e.01
Eel	02/28/62	U-12b.07

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The new schedule was a substantial departure from the most recently revised Livermore schedule. One significant change was Livermore's use of Area 9 for vertical hole shots such as those LASL had been performing in Area 3.

Darol Froman, LASL Technical Associate Director, responded on November 2 to queries by General Betts, getting LASL on the bandwagon by stating that even if atmospheric testing started, LASL urged that a stockpile of underground holes be accumulated in case atmospheric tests were again stopped. He said that any particular number of sites would just be a guess, but perhaps four sites for a megaton, four for 50-100 kt, four for 20 kt, and four for 10 kt. In spite of current casing difficulties, LASL still preferred holes to tunnel sites. Based on recent experience, LASL endorsed the 475 $W^{1/3}$ containment rule for the present time.

Jim Reeves further endorsed some of LASL's position in the November 2 ALOO reply by pushing to obtain a large inventory of both tunnel and vertical hole sites, much greater than existed before the moratorium ended. After Reeves had emphasized what kind of a stockpile of underground sites should be built up, he made an interesting recommendation to Betts, namely "that atmospheric testing be held to a minimum even at the expense of increased costs and acceptable delay in order to decrease to a minimum the probability of public opinion forcing an early termination of atmospheric testing."

The Livermore reply from John Foster on November 2 was slightly more elaborate than the others, still going very clearly down the road of developing extensive tunnel complexes. Livermore estimated that 12 portals would be required to provide a capability for about 50 tunnel site detonations per year, and Foster also pushed consideration of the Christmas Tree concept for higher-yield detonations. Foster did note several diagnostics problem areas requiring development for the tunnel sites, specifying these as:

> **(A) There is need for a technique to give prompt yield data; (B) experiments such as Pinex and Phonex which involve recovery of data relatively close to the detonation point involve high risk of losing data due to rockfalls; (C) large-yield shots (50-100 kilotons) involve very long cable runs and consequent high cost to bring out diagnostic information. LRL is conducting research and development work in each of these areas and this must be continued until acceptable solutions are found.**

He also stated that Livermore preferred tunnels because of the size and the large number of drifts per tunnel portal. He made an interesting and admittedly very gross comparison of the costs and time of preparation for tunnel sites and vertical hole sites, which is shown in Table XXIII.

TABLE XXIII
LIVERMORE COMPARISON OF COSTS AND TIMES FOR UNDERGROUND SITES
November 2, 1961

	Cost (thousand dollars)		Preparation Time (days)	
Yield Range (kt)	Tunnel	Hole	Tunnel	Hole
Less than 1	295	75	30	25
1-5	425	130	45	40
5-10	525	170	55	60
10-25	675	280	70	80
25-30	870	400	85	110

The poor results in collecting useful prompt radiochemical samples on LASL events continued with the Mink event, as reported by Charles Browne at the November 6 LASL Weapons Working Group meeting. The sample that had been obtained, in addition to being highly fractionated, was small in size and Browne concluded that the yield could not be estimated from the sample. Drilling had been started to obtain better samples from the core.

5 U.S.C. 552(b)(3) DOE Ex. 3

Of interest in comparing the Laboratories' diagnostic techniques was a November 9 message from C. Godfrey of Livermore to DMA on the radiochemical yield for Chena, [redacted] Thus, Livermore, by a tunnel technique, claimed to be able to measure yield with 10 percent uncertainty from radiochemical sampling within a month of their shots.

In Betts' November 4 message to the Laboratories on the results of the National Security Council meeting, he noted a change in the quantitative character of the projected Nougat program derived from the Laboratory directors' messages on October 28 and 29. He told the Labs that he now would require detailed revisions to this program before asking for Presidential approval for increased expenditure of nuclear materials, which would be substantially different from that authorized in September. Thus, he asked that Reeves convene the NTS Planning Board to provide a revised detailed listing of diagnostics, cabling, device availability, and other considerations needed for the Nougat schedule through the end of February 1962, requesting these revised Nougat plans by November 9. He noted that Seaborg had indicated that the underground program should be vigorously continued. Curiously, there was absolutely no mention made of atmospheric testing by balloons or other methods at the NTS.

5 U.S.C. 552(b)(1) DOE Ex. 1

In response to Betts' November 4 request, Bradbury sent a new LASL Nougat list of [redacted] tests, of which less than half had previously been approved. The list is shown in Table XXIV. Noting that LASL wished to test the Fisher device next because of the behavior of the Mink device, Bradbury requested DMA approval for that shot to occur on November 16 in hole U-3ah. No dates or exact locations were given for the other tests, since they depended upon the availability of holes and the results of related current tests. The November 11 NTS Planning Board would consider such detailed scheduling.

DMA responded to Bradbury's request on November 10, following some telephone conversations, notifying LASL that the AEC was trying to obtain Presidential approval for the addition of the Fisher event with a proposed date of November 19.

5 U.S.C. 552(b)(1) DOE, Ex. 1

The NTS Planning Board met on November 11 in Albuquerque and Reeves immediately thereafter provided DMA with an up-to-date modified Nougat test list. Some of the details of the new list [redacted] the [redacted] Livermore shots planned through the end of February, [redacted] were now planned for vertical holes in Area 9. The other [redacted] were planned to be done in the i and k tunnels, on which construction had begun since test resumption, and in the b and e tunnel complexes, which still required some recovery work. In addition to the effort required to prepare these underground sites, the Planning Board addressed the Ivanhoe series (Table XXVI), which was to commence underground on March 1 and continue through the end of the fiscal year (June 30, 1962). The Laboratories presented preliminary programs requiring a great deal of additional construction. This was set forth by Reeves to the AEC and to contractors required to prepare the underground sites.

The Livermore program for Ivanhoe resulted in the following projected construction program: 18 separate tunnel sites (some double tunnels) to contain tests up to 100 kt; and more sites in either tunnels or Area 9 holes to contain up to 50 kt. Noting that the Planning Board supported all of this long lead time construction for

TABLE XXIV
LASL NOUGAT TEST LIST
November 4, 1961

Device	Yield (kt)	Nickname	Previously Approved for Nougat

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5 U.S.C. 552 (b)(3)
DOE, EXEMPTION 3

Ivanhoe, Reeves asked his assistant, Milton Rex, to develop cost and time estimates for the overall program since he expected that there would be funding problems and wanted to make DMA aware of these as soon as possible. By the end of November, LASL reported that there were five underground sites complete, with about 20 additional drilling jobs in progress.

On November 14 ALOO published one of their periodic listings of the projects in which they were involved or had done planning. Under the Vela Uniform program they showed the Dribble and Shoal programs as needing lots of work and the Shade program as in progress, including support of DOD measurements-- primarily seismic--at the NTS. The Plowshare program included three active projects; Gnome, discussed elsewhere, and Wagon and Chariot still being addressed. (It is not at all clear what the status of Wagon and Chariot was.) Under DOD support, the Marshmallow effects test involved installation of 800 feet of vacuum pipe in a new tunnel facility, which was being prepared in the Oak Springs tuff of Area 16 at the NTS. Hardhat, which included structural response experiments as well as Vela Uniform experiments, required a tunnel and hole in Area 15.

Reeves continued specifying NTS authorizations on November 14, directing Rex to proceed with construction of U-3ah and with emplacement for the Fisher test (excluding stemming) to meet the November 19 date provided to DMA, though that test did not have final approval. (That approval came only on November 17.) Furthermore, Reeves directed that construction proceed on U-3ao and U-3ad for LASL events Dormouse and Coney on Janary 5 and December 12, respectively, and on Area 9 Holes a, b, c, and d for four new Livermore events. For the longer term, he reiterated the request for the best available time and cost estimates for the overall Ivanhoe effort, charging Rex to coordinate thoroughly preparations for that program at the field level in real time to assure there was continuing need for the facilities.

TABLE XXV
MODIFIED NOUGAT, NTS PLANNING BOARD
November 11, 1961

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TABLE XXVI
LASL UNDERGROUND SITE PROPOSAL THROUGH FY62
(IVANHOE)
November 1961

Quantity	Depth (ft)[a]	Area	Date Required
2	1,000	3	March 1962
2	2,500	New Site	April 1962
2	1,600	3	April 1962[b]
2	1,000	3	May 1962
2	1,200	3	May 1962[b]
1	2,200	3	June 1962
3	200	3	March 1962
1	6,000	U-15d	June 1962

[a]All holes were to be 3 feet in diameter.

[b]These may be heavily instrumented, involving shaft construction and room or drift at bottom of shaft and several auxiliary holes.

These proposed expanded schedules for Nougat were discussed by the Commission on November 16. In discussing the feasibility of supporting such an expanded schedule, Betts said that ALOO "has unqualifiedly indicated the necessary increase can be achieved if vertical shafts can be utilized; it is less certain if tunnels are required." Betts also noted that as a result of the Chena event in U-12b.09 there was danger of excessive radiation exposure to the miners now attempting to clean out that tunnel for further testing. The then current dose limits recommended by the Federal Radiation Council (FRC) were 3 rem per quarter and 5 rem per year; 3.9 rem quarterly and no yearly limitation had been used on the most recent EPG test series. The General Manager noted that few of the miners working in the tunnel had exceeded the quarterly limit of the FRC, but in view of the danger of a large exposure at some point in the tunnel, it was uncertain at what pace the tunnel could be cleared. Discussion of funding for testing in the same meeting included concern expressed by Commissioner Wilson about Betts' remark that funds might have to be diverted from the underground program to initiate preparations for atmospheric testing. Betts explained that funds taken from the underground program would be redirected only with assurance that they could be replaced later, thus allowing the underground program to continue as scheduled while expediting the atmospheric test program.

The frank opinion of one of the experts in the testing community, Bill Ogle, on the greatly expanded underground programs being readied in parallel with the atmospheric test preparations, was expressed in an internal LASL memorandum of November 17, "Outlook for Nougat/Ivanhoe." In the transmittal letter attached to a listing of [redacted] devices considered for testing before the end of February 1962 was the following:

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> In all the wild dreaming going on recently in the weapon test business, a set of devices, holes, etc., have been associated with Operation Nougat in order to obtain authority to do field construction, expend active material, etc. . . . I am unaware of anyone who believes there is any serious relationship between this listing and what will actually happen. However, for what it is worth, the appended table is presented. (Ed. note: table not included here.)

Livermore's overall projections were even more grandiose, in addition to their relying most heavily on a tunnel testing technique which had had very poor success to date.

By November 10 one possible method of alleviating the cable shortage problem had been tried and found not to work. The United Kingdom, which had a large amount of surplus cable, had offered this--through Headquarters, AEC--to the test community. However, after an exchange of information and correspondence Reeves informed Betts that none of the Laboratories had a practical use for the British cable, which had originally been offered about a month earlier.

Adding to the problems of achieving the ever-growing program at the NTS was the fact, discussed at a Commission meeting on November 21, that the pipe fitters and operating engineers at the Site had had a disagreement and the pipe fitters had gone on strike, establishing a picket line which certain of the other unions had refused to cross. Luedecke noted that the Rover project was being delayed on a day-by-day basis and the weapons test program would be seriously affected.

The expanded Nougat test program was sent by Betts to Chairman Seaborg on November 28 along with a proposed letter to the President requesting authorization.

Radioactive Contamination of Tunnels

A brief description of the tunnel contamination problems was included in a letter from General Manager Luedecke to the Chairman of the JCAE, Chet Holifield, on November 29. Luedecke pointed out that the Antler test:

> . . . resulted in loss of entry into the U-12e tunnel complex because of the contamination and spread of debris through the side drift and the main tunnel. . . . Following the Antler shot, a program was commenced to decontaminate and rehabilitate the U-12e tunnel complex so as to attempt to salvage four shot locations which had been previously constructed. . . . U-12e tunnel rehabilitation has been accomplished to a point about 1,800 feet from the portal. At this point, the tunnel is plugged by a mass of debris from the side drift where the Antler device was placed. It has been determined that rather than attempt to remove the debris plug, it should be bypassed and work is proceeding. Until we are able to complete the bypass, we will not know whether the remaining shot sites of the U-12e tunnel can be used. . . . In addition to the difficult situation in regard to the U-12e tunnel complex, we have a troublesome problem with regard to tritium in a portion of the U-12b tunnel complex. This problem arose after the Chena event and was complicated by the tritium residue remaining from the Evans event of Hardtack II. The combination of the external radiation whole body exposures in the U-12e tunnel and the additional internal exposure in the U-12b tunnel has resulted in 108 miners and supporting personnel receiving, as of November 24, 1961, a combined whole body exposure in excess of 3 rem in one quarter and of these, 38 in excess of 5 rem in one year. No individual was exposed to more than 8.045 rem. . . . Because of our inability to continue full-scale tunnel operations within the established normal peacetime radiation criteria, the U-12b and U-12e tunnel operations were curtailed on November 27, 1961. Underground workers who were approaching 3 rem per quarter were removed from the tunnels, as well as any individuals who may have received a dosage of more than 3 rem per quarter.

What would this do to the expanded Livermore plans for building up a large stockpile of tunnel sites? Part of the answer was included in a message the same day from Cliff Bacigalupi of Livermore to Jim Reeves which stated, "Reentry into the E tunnel complex indicates that the U-12e.01 and U-12e.04 locations cannot be used for the Cimarron and Brazos events in the near future. In order to maintain the Nougat schedule, it will be necessary to develop new emplacement locations for these events." Thus, he requested approval for construction of new sites in Area 9.

In light of these problems, it seems that the death knell for the Christmas Tree concept could already be heard. The Christmas Tree working group had continued to meet through November, and on November 9 H&N had submitted preliminary cost estimates for various sites which were being evaluated for this complex. The estimates for seven possible locations in Utah, California, and New Mexico ranged from about $5 million to $8 million. By the end of November a final draft of the Christmas Tree report was being reviewed by the committee and other concerned people. LASL had made it clear that while they were not interested in developing the concept, they would continue to evaluate it and keep informed in order to be ready to utilize it if it proved feasible.

More Nougat

LASL finally got a chance to see if the radiochemical samples obtained by drilling back would give better results than the prompt samples and other techniques. On November 17, drillback of the first LASL shot, Shrew, brought some samples to the surface. Charles Browne reported the observations and analysis results to the LASL Weapons Working Group on November 27:

> The general activity level remained constant in the hole from a depth of 310 feet down to a 3-foot void encountered at 335 feet. The activity then jumped by a factor of 10 or more at the void, returned to the initial level from 338 feet to 343 feet, then fell off to zero at about 350 feet. Data was taken from samples obtained around 325 feet, at the level of peak activity, and at 343 feet. The fractionation pattern appeared reasonable, with very little fractionation at the 330-foot level, an excess of volatiles at the upper levels, and an excess of refractories at the lower levels. [redacted] The drillback on the Mink hole is now down to 425 feet. Complete samples are expected sometime this week.

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The value of these more accurate results was immediately evident from a discussion opened by Harold Agnew on corrections which might be made to the [redacted] on the basis of the Shrew yield results. [redacted] At the same meeting, the upcoming schedule presented by Ogle listed Fisher to be shot sometime in the next week, followed by the next two or three shots at about one-week intervals.

> Holes exist for Packrat, Ferret, and Coney, with only surface work remaining to be done. The hole for Ringtail is not yet available. Subsequent to the discussion of the [redacted] .. it was noted that, in order to fire Ringtail in December, it might be necessary to skip one of the preceding shots. Ferret could be delayed if necessary. The present order of events then is Fisher, Packrat, Ringtail, then Ferret and Coney, all hopefully before the first of the year.

When the time to perform Fisher finally did arrive in late November, following delays because of strikes at the Test Site, the event was postponed due to a mechanical problem discovered during insertion. The shot was first slipped to December 1, then December 2, and then postponed 24 more hours because of flooding and danger of shorting a transformer at the alpha station. Although electrical problems were still present on the morning of the test, it was detonated on December 3. It [redacted] involved new diagnostic techniques, and was detonated about 1,200 feet deep with essentially 100 percent containment. It resulted in the largest subsidence crater yet. The crater, which formed 27 minutes after the event, was 50 feet deep and 585 feet in diameter. Furthermore, as Charles Browne reported at the

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WWG meeting on December 13, prompt sampling yielded the best sample yet recovered by that technique, the sample pipe data showing betw[redacted]fissions. Al Graves, the Scientific Advisor to the Test Manager, reported immediately after Fisher that the crater "now contains rubble from one red shack, one white shack, one red and white shack, one blue shack, one silver shack, and one Chic Sale." One of the lessons learned from the extensive cratering following the Fisher event, as can be seen in Figure 12 (a, b, and c) was the possibility of damage and loss of equipment. Three amplifiers and a power supply were completely destroyed and a zero rack and other things suffered lesser damages.

The Nougat schedule as recently revised had not yet been fully approved, and it was still loosely defined. LASL was reconsidering the Nougat schedule in order to conduct those experiments relevant to Blue Straw (the Pacific Operation) as soon as possible, and reached the conclusion that those tests leading to a [redacted] were to have first priority. The devices considered important as candidates for this role were [redacted] The first of these was to be tested in the Ringtail event and that shot was moved ahead to utilize the existing Packrat hole, which was 200 feet deeper than the 1,000-foot depth planned for Ringtail. This switch necessitated several days delay in Ringtail, which was scheduled as the next LASL event on December 20. That change left Livermore's initial vertical hole test, Mad, as the next event. The AEC Chairman approved Mad on December 8 and it was performed on December 13. It was the first Livermore event of this series to be completely contained.

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Toward Normalcy in Nevada

On December 12 General Betts formally requested from the Laboratories a reassessment of their underground programs in the light of two main assumptions: (1) that the underground program would not be terminated in the near future so that efficiency of operations might become a primary factor, and (2) that the atmospheric test program would begin about April 1 and consist of a program approximately as presented to the President on November 30 (see Chapter IV). Betts requested that the Laboratories provide detailed event plans, including firing location, device readiness date, etc., for their programs through the end of March, and he wished these to be sent to him by April 1. Moreover, he requested a tentative list of additional tests (with less detail) covering the April 1 to June 30 period. Concurrently, Betts asked Reeves to have the NTS Planning Board consider the feasibility of providing suitable locations for each of the suggested Laboratory tests. As a guideline, with no clarifying elaboration, Betts said this should be based on "resuming a normal, noncrash set of operating conditions at the NTS." Following receipt of the desired information from the Laboratories and Planning Board, Betts would request approval for the revised schedule through April 1, 1962. In another message from Betts to Kenner Hertford on the same day, the budget problems arising from the enlarged underground program projections were evident. Betts reported that because more money was needed for the larger underground program, the AEC had, for the time being, deleted a substantial number of the items in the Ivanhoe program.

Some clarification of DMA guidelines was provided by Reeves prior to a December 20 NTS Planning Board meeting. Specifically, the definition of "normal, noncrash set of operating conditions" was to be construed as a 40-hour workweek for all crafts where possible, but providing for three-shift operation for those functions which normally work on that basis. For the Test Site technical and management staff, a 54-hour workweek was considered normal during a test series. In addition, the radiation exposure standards to be utilized in NTS operation would be to limit whole body

~~SECRET~~

Figure 12.
(a) Part of teletype from Al Graves, Scientific Advisor for the Fisher event, to the Test Manager, referring to above photograph of the Fisher event.

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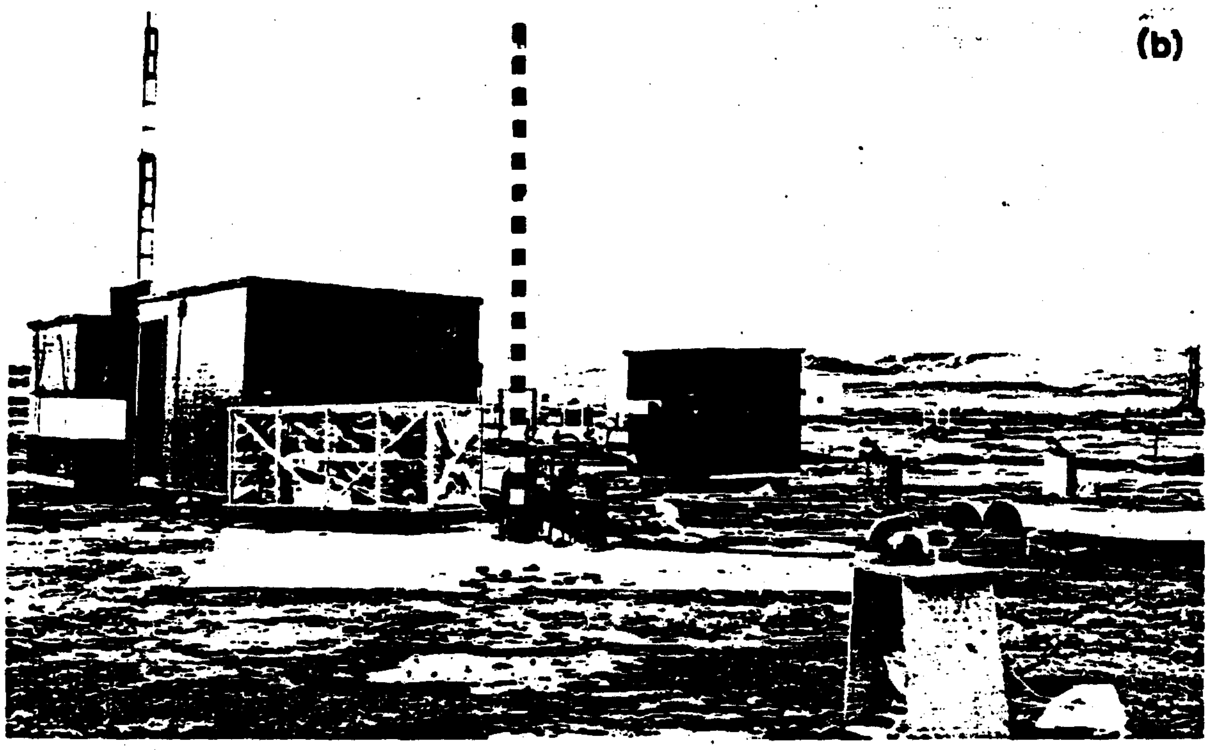
FM GRAVES SCIENTIFIC ADVISOR

TO REEVES, AEC TEST MANAGER

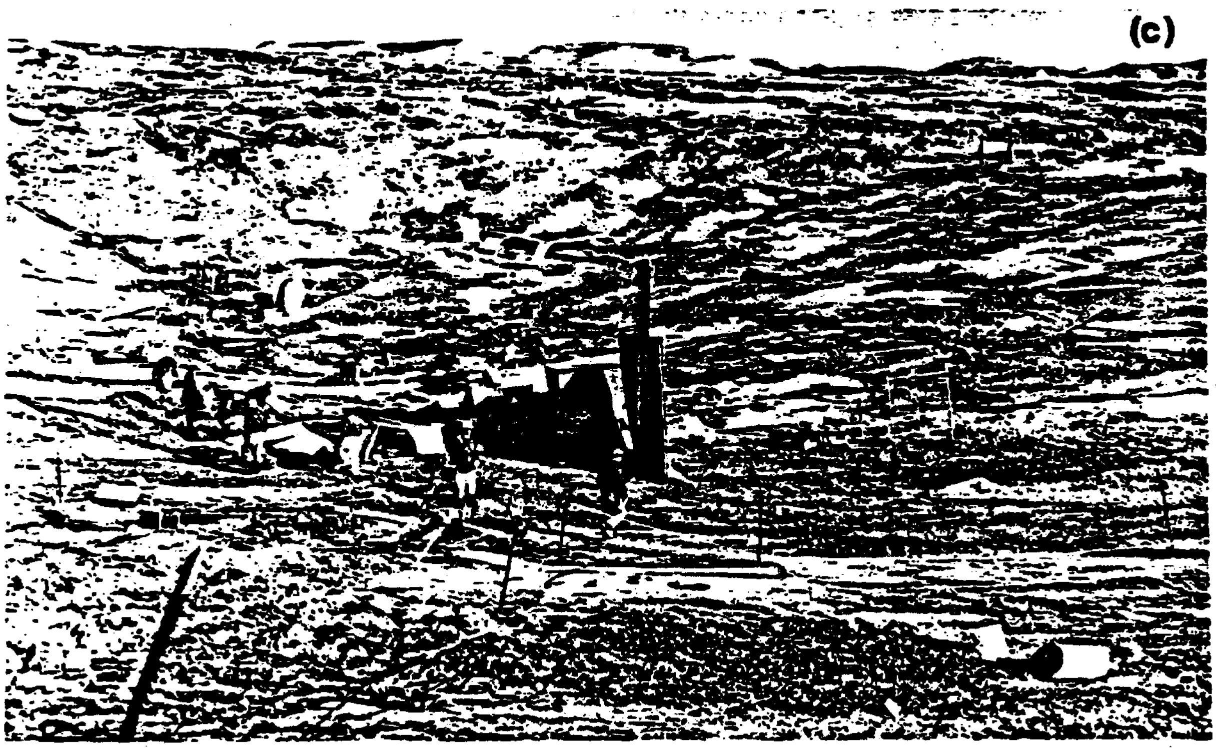
THE CRATER NOW CONTAINS RUBBLE FROM ONE RED SHACK, ONE WHITE SHACK, ONE RED AND WHITE SHACK, ONE BLUE SHACK, ONE SILVER SHACK, AND ONE CHIC SALE.

~~SECRET~~

Figure 12. (cont)



(b) Close-up view of Fisher buildings before detonation.



(c) Close-up view of some of the same Fisher buildings after collapse.

radiation to 3 rem per quarter and 12 rem per year, provided the quantity 5 times (N minus 18) rem, where N is the individual's age, was not exceeded. (Ed. note: Obviously, it was assumed that there were no workers younger than age 19.)

Improvement in LASL's ability to obtain delayed radiochemical samples for yield determination was reported by Charles Browne at the WWG on December 13. Drillback samples on Shrew, Boomer, and Mink had been obtained and second drillback holes were being started. Drillback on the Fisher event was already at 600 feet, and the average drilling rate was about 100 feet per day. Thus, techniques using the newly acquired rigs had been successful in greatly accelerating the acquisition of postshot radiochemical samples. Two days later, on December 15, the drillback went 190 feet in one day, the highest rate per day yet attained.

The W-, J-, and T-Divisions within LASL got together on December 14 to modify the underground test schedules in light of the atmospheric test considerations and the test results to date. Norris Bradbury communicated this revised list to General Betts late that day. It was based on roughly ten days between shots (with the exception of one-point safety tests) [redacted]

On the same day, December 14, Foster sent Livermore's revised list to Betts. [redacted] Substantial use of the tunnel areas was still planned as shown in the attached Table XXVII. This table shows the pertinent information for both the LASL and Livermore shots as presented to Betts. As noted several days later by Reeves, the urgent need for integrated answers made it impractical to assemble the Planning Board in time to meet DMA needs. Consequently, a later Planning Board session would include review and refinement of these schedules, but that session would not be held until general concurrence and approval of the schedule had been received from Washington.

On December 5, in discussion between Hans Bethe and Bradbury, Bethe indicated his preference for tunnel testing over vertical holes. These comments led to a December 15 memorandum from George Cowan, the LASL radiochemistry group leader, to Bradbury, summarizing the status of radiochemical sampling from vertical hole tests. The memorandum notes that experience on prompt samples had been very poor, but that the Fisher event had finally yielded the first useful prompt sample [redacted] Delayed sampling to that date had had one- to two-months delay, but with the latest equipment this delay was coming down to a matter of weeks. This shorter delay permitted reasonably accurate determination of yield [redacted] LASL had had greater drillback problems than Livermore because it was more difficult to drill in uncompacted alluvium than in compacted tuff. Overall, however, Cowan felt that Bethe's conclusions were based on overly-optimistic statements about tunnel containment, statements not in accord with experience to date, and that the only significant advantage of tunnels over holes was the ease of some line-of-sight experiments, rather than in sample recovery. Bradbury utilized Cowan's summary in his December 20 reply to Hans Bethe comparing tunnels and holes for underground testing in which the desired results were primarily yield, alpha, and some timing information. Bradbury noted that so far the tunnel shots had all given trouble to a greater or lesser

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Withheld Under 5 U.S.C. 552 (b)(3) DOE Ex. 3

TABLE XXVII
LASL AND LIVERMORE PROPOSED NOUGAT SCHEDULES
December 14, 1961

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DOE, Exemption 3

degree with their lack of containment, whereas the vertical holes had given no such trouble. This was particularly significant from a personnel exposure point of view. LASL had also been able to fire about twice as many shots as Livermore and all of LASL's shots had and would have prompt diagnostics. Bradbury did agree that for heavily diagnosed, heavily collimated experiments, tunnels were probably preferable to vertical holes. Further, he observed that Bethe's point was probably not that tunnels were so much better than vertical holes, but that LASL should believe "more fervently in the virtues of underground testing." While acknowledging the place of underground testing, Bradbury expounded on his strong feelings about preparing and carrying out atmospheric tests, feelings based on his perception of the realities of the international situation.

Since there could be no Planning Board meeting in the time required to submit the revised schedule (noted earlier), Reeves felt that the separately determined and submitted Laboratory schedules should be reviewed with those who had to support them. This occurred in a meeting on December 16 and 17 with the best qualified personnel of his office, H&N, and REECo. The result was concurrence that the proposed Lab schedules were practical and could be done efficiently at a cost of $121 million in FY 1962 and $28 million in FY 1963. Although they clearly felt themselves under some pressure to reduce the overtime costs and cut down the number of people working many extra hours per week, the contractors made a case for continuing the drilling program through the summer using three crews, each crew working seven days per week, eight hours per day. Part of the justification, other than that it was required to meet the Lab schedules, was the cost of the rented drill rigs (they were approximately 40 in number) and the impracticality of shutting down rigs on weekends (because of problems such as mud circulation). Moreover, adding another shift to decrease the hours per man would have greatly increased the numbers that would have to be supported with living facilities, thus causing other indirect costs and problems at the Test Site. The same sorts of arguments applied for the crews involved in tunneling, where an average workweek of 54 hours for the present crews would still require hiring additional men to meet the Laboratories' programs.

LASL detonated its fifth Nougat event, Ringtail, on December 17, and the good record for containment continued: containment was essentially complete, with the highest radiation levels being near the prompt sampling pots and no radiation outside the immediate test area. Diagnostics results were discussed in a WWG meeting three days later. Westervelt reported that the J-10 alpha station had gotten results which had been analyzed partially. Charles Browne reported that, in contrast to Fisher, there was no indication of a good prompt rad-chem sample. Drillback had begun some distance from the well head and a new (to the NTS) technique known as Whipstock drilling was being used, in which the direction of drilling may be altered during the operation to intersect the original hole at the appropriate depth. A brief summary of LASL progress in radiochemical sampling techniques for underground shots was provided at the last WWG meeting of 1961 on December 27. George Cowan summarized the results from the five Nougat shots as follows:

> Analysis of drillback samples gave a radiochemical yield o [redacted] Boomer samples were not yet analysed. Mink drillback samples gave a radiochemical yield of [redacted] Fisher had the first useful prompt sample, which showed the yield to be greater than [redacted] Ringtail had brought no results as yet.

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Livermore's containment problems continued on the Feather event on December 22. Immediately after firing, a small cloud came out of the tunnel portal (U-12b) and the vent pipes on top of the mesa also expelled a cloud. Activity half an hour after the shot was about 3 R per hour at the portal and the vents, and off-site activity was not anticipated. In spite of the venting it appeared that early recoveries of film could be accomplished. Six hours after the test it was reported that diagnostic data had been recovered from all stations outside the portal, but reentry would not start until about January 3, 1962.

J-15, the LASL hydrodynamic group, reported just before the end of the year that the Fisher event had been the first one instrumented for time-of-arrival measurements. These measurements, together with data on the media and the equations of state, would, with time, lead to improvements in this yield technique. As of December 22, about three weeks after the shot, J-15 analysis of these measurements gave an estimated yield of 13 plus or minus 4 kt.

Livermore Rethinks Tunnels vs. Holes

Livermore, in a message from Roger Batzel to General Betts on December 19, took stock of their present experience in stemming tunnel detonations and outlinged some new ground rules they felt were valid. From their point of view, looking back over not just Nougat but all the tunnel experience, there had now been four detonations [redacted] three of which (Rainier, Blanca, and Logan) had been successfully contained. The fourth (Antler) vented. [redacted] there had been three events, all of which had released significant radioactivity into the tunnel (Tamalpais, Evans, and Chena). As for Chena, since they were able to reenter the b tunnel within a few days after the event, Livermore considered it acceptably contained. Thus, their experience had led them to conclude [redacted] and they felt the underground program should utilize a combination of tunnels and holes in an appropriate combination. In what was certainly a new approach for Livermore, Batzel said:

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> We believe that the optimum manner to use tunnels is one in which a single shot site is constructed for each separate tunnel entrance, and then, depending on the subsequent situation on radioactivity, we would either reuse the individual tunnel for a following event or, if excessive radiation levels exist, we would abandon that tunnel for the time being and let the radiation decay to an acceptable level before reuse. . . . Based on our past experience, we would expect that 60 to 70 percent of the time, a given tunnel could be reentered without delay. We are convinced that given another six months to one year of (Ed. note: "of" inserted) experience, we can significantly improve the probability of successful stemming.

In a statement somewhat in conflict with the Livermore Director's recent statements about being device limited, Batzel added: "It should be remembered that as of the present time and probably at least through the first of May, we are still site limited and some important experiments which are ready for execution cannot be done because of the lack of sites and tunnels." Batzel then turned to the vertical holes which he called an important complement to tunnels, especially for those shots which required a minimum of diagnostics. Livermore was not only expanding the Area 9 hole operation, but also was doing preliminary planning for another area along the west side of Rainier mesa where the water table problems for deep holes might be more simply solved. Moreover, Livermore was discussing vertical hole drilling with a limited number of outside contractors, such as Kerr-McGee. Finally, in answer to a recent query from General Betts about the expensive high-yield test methods of the Christmas Tree concept, Batzel stated:

> In light of our existing experience, LRL also has reservations about the feasibility and desirability of starting construction of a Christmas Tree facility at this time. As we gain more experience about the problems of stemming, radii of damage, and the effects of geology, we will be in a position to make a firm recommendation.

Finally, he expressed the Livermore position that developing a space testing capability was very important, adding that they were concentrating on plans to develop diagnostic techniques and instrumentation for that regime, and it was urgent that the techniques be checked out during the forthcoming atmospheric tests.

Other Nevada Topics

Vela Uniform

Further information on the status of the Vela Uniform program was provided to DMA by Jim Reeves on December 21. The off-NTS program (Dribble) was being maintained in a "bare standby condition for a period of 6 to 12 months." The Shoal event was being treated in a special manner aiming for preparations for a shot, perhaps in

1964. Note was made of the Whirlaway devices prepared for the moratorium Vela program, which were being stored and maintained by Sandia. Reeves asked DMA for further guidance or concurrence with the present status.

Intentional Venting Study

At about this same time, on December 22, Betts raised a question on the utility and effectiveness of intentional venting as a means of alleviating the contamination problems encountered to date, especially in tunnels. Specifically, he asked for comments and recommendations concerning detonation of underground shots so as to form high chimneys which would produce slight intentional venting, thus reducing tunnel contamination.

For a brief period Graves endorsed this as a method of decreasing the time spent in drilling holes for emplacement and sample recovery, feeling that it might be worth investigating in order to establish some assurance that intentional venting could be accomplished without undue risk of off-site radiation. Don Shuster of Sandia said he was not optimistic about using controlled chimneying to lessen tunnel contamination. For one thing, it was hard to plan for chimneying soon enough to relieve pressures on the stemming, thus controlling the venting in the desired method. Batzel answered a little more elaborately, including in his remarks some discussion of the major mechanisms of containment failure in tunnels. In the Antler failure, where water in the hot cavity resulted in a steam explosion, controlled venting probably would have prevented the contamination of e tunnel. [redacted] All of these items, including the debates about the depth of burial scaling constant and the exponent on the yield, illustrated the fact that containment design principles and the mechanisms of venting were not fully understood and were complicated by the various media and site locations. Underground testing was very much in the learning stages.

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DOE, Exemption 1

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DOE, EXEMPTION 1

Nougat in 1962

As an aside, it is interesting to note that by the beginning of 1962, the numbers of personnel involved in NTS activities had increased significantly. Employment by the 25 or more contractors had grown from a total figure of 5,326 on November 11 to 6,454 on December 9, 1961. The growth was almost entirely accounted for by REECo, which grew from 3,865 to 4,910 in this period. Not all of these were in residence at the NTS, but that segment had grown from 2,997 on November 11 to 3,840 on December 9. As of December 11, there were only 150 unassigned housing spaces at the Test Site, and on that date REECo was processing 153 new hires. Thus, all available housing was committed. Another example of confusion resulting from the very busy work schedule involved changes in the construction work scheduling which arose from discussion among the NTS AEC staff and the construction contractors, but not including Laboratory representatives. This type of confusion led to strong objections being voiced by the Laboratory representatives at the January 3, 1962, meeting of the NTS Planning Board. The result, of course, was agreement among all participants that, subsequently, any changes in construction activities would be coordinated with the Laboratories. A further result of the meeting was a new schedule, extending through the end of March 1962; see Table XXVIII.

Also early in the year, Livermore summarized their diagnostics results to date:

> On the Antler event, radiochemistry gave a yield of 2.4[redacted] the other diagnostics were to be done by equipment installed in the tunnel, in the interest of minimizing the time to get ready, and due to the postshot steam explosion this fast electronic data was not recovered.
>
> The Chena event resulted in radiochemistry indicating a total yield[redacted]
>
> The Gnome event in the Plowshare program had as one of its objectives gathering diagnostic information on a Ditchdigger configuration. Diagnostic data was by and large not yet available since the radiochemical sample obtained was still being analysed and gaseous fission products released from the main shaft fogged some of the fast electronic film, making it then appear that data on the primary had been lost. Some data on the secondary had been salvaged and was being analysed.
>
> On the Mad event a radiochemical sample was obtained and it was then being analysed.[redacted]
>
> The Feather event had a yield[redacted]

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On January 10 General Betts informed the test organization that the newly revised Nougat schedule through the end of March had been approved by the President, who also had approved the required expenditure of special material. The only event omitted was Pampas, the detonation for the United Kingdom, which would be handled separately.

[redacted]

TABLE XXVIII
UNDERGROUND SCHEDULE, NTS PLANNING BOARD
January 3, 1962

Nickname	Sponsor	Location	Ready Date
Stoat	LASL	U-3ap	01/09/62
Agouti	LASL	U-3at	01/19/62
Anteater	LASL	U-3ao	01/29/62
Codsaw	LRL	U-9e	01/31/62
Platte	LRL	U-12k.01	02/07/62
Rogue	LRL	U-9g	02/07/62
Dormouse	LASL	U-3ai	02/08/62
Coney	LASL	U-3ad	02/13/62
Cimarron	LRL	U-9d	02/14/62
Brazos	LRL	U-9h	02/28/62
Hardhat	DOD	U-15a	01/15/62
Packrat	LASL	U-3aq	?
Pampas	LASL/U.K.	U-3al	02/28/62
Aardvark	LASL	U-4b	03/03/62
Jordan	LRL	U-12i.01	03/07/62
Kuskokwim	LRL	U-9r	03/07/62
Armadillo	LASL	U-3ar	03/10/62
Cheyenne	LRL	U-9b	03/15/62
Ermine	LASL	U-3av	03/15/62
Lemming	LASL	U-3ax	03/20/62
Chipmunk	LASL	U-3ay	03/25/62
Columbia	LRL	U-9i	03/28/62
Des Moines	LRL	U-12j.01	03/28/62
Chinchilla	LASL	U-3az	03/30/62

The January 10 Nougat schedule as approved through the end of March was to prove just as changeable as earlier schedules. Of the ten shots authorized through the end of February, only three of the nine shots that were actually performed in that time period were from the DMA authorization list.

LASL reported on January 12 that no drillback sample from Fisher had yet been obtained (about 40 days after the test). Production and release of steam in the drillback operations had caused the delay in obtaining core samples. No prompt samples were obtained in Ringtail, [redacted] The latest test, Stoat, on January 9, also failed to give prompt samples, [redacted] indicated a yield of about 3 kt. Both Stoat and Ringtail had been detonated in holes deeper than required in the interest of doing the tests as early as possible and using the results in designs to be tested in the atmospheric test program.

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It could be seen at this time that the Livermore testing, which had started out the postmoratorium period with more potential shot sites, now was behind because of the unanticipated loss of sites arising from contamination and containment problems. LRL thus was now using a hybrid of holes and tunnels. LASL, which was pursuing its program with essentially the same diagnostics they had used during September 1961, was beginning to improve techniques of gathering samples and obtaining data (e.g., a portable alpha station had been prepared and was in use by the beginning of February), and was preparing for future events. Livermore, which had the more elaborate early plans for diagnostics, were not yet getting much data from their instruments because of containment problems.

The Danny Boy cratering test was taking some shape by mid-January, as indicated by a January 16 message from Reeves to Betts discussing the dosage predictions for this test. Of particular interest is that the worst case prediction for some of the Danny Boy experiments showed that the project would get such a heavy dose that postshot work would be seriously delayed for many weeks. Reeves asked DMA to let him know immediately if Washington planned to specifically direct execution of this event, so that he could call the NTS Planning Board into session to evaluate potential problems.

Some of the improved sample drillback techniques evidently paid off because the first drillback sample of the Stoat event (fired at 1000-ft depth) was received only nine days after the event. Two other indicators of the improving test situation are noteworthy; namely, by late January a supply of vertical holes had been built up, with six holes available ranging in depth from 200 to 1,200 feet; and EG&G was preparing two "universal zero racks" which would be delivered to Los Alamos and Livermore the week of February 5.

In early 1962 planning for the DOD effects test Small Boy began, and this was followed not long after by inclusion of other planned atmospheric tests for the DOD at the NTS. Much of the DOD test program that evolved into, among others, the Little Feller and Johnnie Boy events, came from a program Gerry Johnson (Assistant to the Secretary of Defense for Atomic Energy) discussed with DASA in January 1962. Johnson suggested tests at the NTS to obtain data on which to base an improved theory of cratering produced by nuclear explosives. He wanted an estimate of the program's scope no later than February 26. The tentative program suggested to DASA noted that the shots might include a 100- to 120-foot-deep test in basalt, a 20-foot-deep test to address scaling laws, a series of three tests (1 foot above the surface, on the surface, and 1 foot below the surface) to assess the dependence of ground shock and cratering effects on height at the surface, and a 50- to 100-foot-deep test in dolomite. He proposed to begin as early as March 1.

On March 7 Don Schueler of the Livermore NTS organization, in a letter to Fred Hohner of the AEC reviewing the drilling operations, noted, as background, that the REECo drilling division had been born after the dissolution of the relationship between Livermore and the E. J. Longyear Company (who had done the Livermore drilling) in 1958, with most of the equipment and pertinent supplies being retained by Livermore or the AEC. Schueler recalled a number of problems with REECo's capability since the Nougat program began, noted the Livermore opinion that REECo was not staffed or equipped to accept the increased load resulting from the beginning of Nougat, made recommendations for improving the REECo capability, and suggested letting other contracts for drilling work. After Livermore began its massive vertical hole effort in Area 9 in mid-October of 1961, problems with REECo led Livermore to recommend that a single agency be made responsible for the drill work and that that agency be H&N. Subsequent discussions led to the decision that vertical emplacement holes would be drilled by a new contractor, and REECo would retain responsibility for all other drilling, e.g., sampling drillback and satellite holes for hydrodynamic yield measurements.

On March 13, Vay Shelton and R. Preston of Livermore distributed a paper entitled "Technical concept for a program of measurements of phenomenon [sic] involved in nuclear explosions in tunnels." Based on their experience in tunnel shots, as pointed out in the paper, LRL planned additional measures to improve stemming and to control venting, incorporating these in future tests beginning with Platte. The paper described in some detail a modest program of measurements to evaluate the effectiveness of the modifications. It was noted that for shots which go below the predicted yield, lack of containment was not unexpected when the stemming had been designed to operate properly for the predicted yield. Consequently, "to obtain more assurance that frizzles do not occur, LRL is now 'pretesting' nuclear devices with vertically emplaced test detonations in the Yucca Flat alluvium." The authors note two ways in which destructive shock waves can be generated in tunnels: for shots, such as Antler, that give the predicted yield, the collapse of the cavity formed by the nuclear explosion can generate a shock wave; and in other shots, collapse of a portion of a tunnel can generate a shock. In both instances the shock can dislodge sandbags and other types of plugs, leading to venting. The Laboratory planned to institute a series of measurements, in conjunction with SRI, (a) to study the phenomena involved in venting and other aspects of containment, (b) to evaluate containment effectiveness, and (c) to ensure the safety of postshot operations.

On March 21, Al Embry of LASL J-Division reported on some interesting cable EMP experiments conducted on the LASL Mink, Fisher, Ringtail, and Stoat events. The overall purpose of Embry's experiments was to characterize the EMP signals induced in cables positioned in various ways around the nuclear explosion and to investigate the potential diagnostic applications of such signals, especially for measuring time intervals in multistage devices. On the first test, Mink, three methods were tried, namely, wrapping cabling around the sampling pipe, putting cabling around the zero point on the surface, and utilizing cables run down the hole to the device canister. Only the last method showed promise and there were further experiments on the next three events. Embry summarized his results as follows:

> Very large clean signals are induced in coaxial cables near the device. These signals are short, the main signal usually being about one-tenth microsecond wide, and with no signals after about one microsecond. Time intervals of one microsecond or more should be very easily measured from these signals, and much shorter ones with reasonable care and proper equipment.

In an independent effort, other LASL scientists (especially L. K. Neher) had by now found that signals that followed the temporal behavior of the gamma radiation output were induced in cables due to the Compton effect. Thus, Neher had been able to make alpha measurements using so-called "Compton diodes," which then rapidly replaced conventional fluor-photodiode detectors in reaction-history (alpha) measurements. LASL pursued the refinement of solid Compton diodes, which in every respect (linear response, appropriate sensitivity, wide dynamic range), were superior to their predecessors, making the reaction-history measurement, even on boosted devices, a precise "table top" experiment. (Later it was learned that a Brazilian patent existed, based on the same principle.) Some time later, as they too moved toward vertical holes, LRL introduced a "vacuum Compton diode" into their program. Neither laboratory accepted the other's approach to alpha detector technology, though each vastly improved the quality of close-in reaction-history measurement. Later developments were to expand the utility of underground testing in vertical emplacement holes beyond any 1962 expectations, rendering the early tunnel versus hole debate most unimportant.

Overview of Nougat

The resumption of testing in Nevada should be reviewed in order to emphasize the enormous effort made in achieving the testing rates and quality of technical experimentation in such a short time.

The LASL review of underground testing in 1962 showed that while the techniques had not been thoroughly developed at the beginning of the series, a capabilility for obtaining good data existed by the summer of 1962.

> The observation of the yield enhancement effect, which is caused by neutrons reflected from the walls of the test hole causing extraneous fissions in the explosion process, caused some initial concern about the accuracy of the radiochemical yields. Although the effect has been reduced by the use of appropriate shielding materials, we are not yet able to make very satisfactory yield determination, particularly of all-oralloy devices where the accuracy is no better than plus or minus 20 percent. The data on alpha and time intervals are, of course, fully satisfactory.

Balloon Shots at NTS, Early 1962

On January 5, General Betts notified the Laboratories and Operations Offices of a slightly new twist on atmospheric test possibilities, which arose from certain political aspects of resuming testing. It had been suggested that it might now be desirable to open the U.S. atmospheric testing program with one balloon shot executed at the NTS on March 1. Therefore, Betts requested that each Lab submit one or more candidates for this single shot and provide assurance that each candidate could be readied and executed by March 1. Candidates might come from either the current underground test program or atmospheric plans, and it was desirable, although not necessary, that the yield should not exceed 10 kt. The contingencies were to be coordinated with Reeves to assure that the NTS could support the candidates. Betts added that, assuming the March 1 date for the NTS balloon shot, it might then be desirable to execute one of the Pacific shots as soon as possible thereafter, but not before March 15. Although he did not know whether JTF-8 could support a shot on that time scale, he assumed it to be no problem and wanted one or more candidates from each Lab for that shot.

On January 7, Batzel presented two Livermore candidates of less than 10-kt yield, both of which could be ready for a March 1 shot. The LRL facilities in Area 9 would require only minor rehabilitation, and Livermore's examination of drilling schedules indicated that an atmospheric detonation in Area 9 would lead to only minor adjustments in the schedules. Thus, Livermore could meet the March 1 date with a high degree of assurance for either device in their Area 9 balloon facility.

On January 8, Bradbury presented four LASL possibilities in the proper yield range for a balloon test by March 1. Moreover, LASL had four devices that could be ready for overseas airdrop by March 15. For a single balloon test at the NTS, LASL would not propose to set up even minimum diagnostics stations, but Bradbury also pointed out that if any balloon test was to be done in Nevada it would seem logical to continue such balloon testing. If that became the case, LASL would need to know at the earliest possible date so as to prepare for such testing in Area 7.

Betts told the Commission that approval by February 9 was necessary so that the site and diagnostics could be ready by March 1. The Chairman said that he would advise the White House a decision by February 9 was needed. On the next day Betts informed the test organization that he had recommended beginning preparations immediately using a LASL device. He did not think the Commission would approve this recommendation soon enough to allow for a March 1 shot even on a rush basis, but, pending Commission action, he personally requested that the necessary preparations be made, except for practice balloon flights.

Of peripheral interest in these arguments were discussions of the public relations approach to test resumption announcements. The information officers of various agencies met with the President's Press Secretary, Pierre Salinger, in early February and generally agreed on some of the recommendations of the Foster Committee. The Foster Committee had recommended to the President that he should consider resuming atmospheric testing with a shot in Nevada since that would help reduce the concern about putting fallout only in other countries and it would make atmospheric test resumption possible before the opening of an upcoming Disarmament Conference. McGeorge Bundy, the President's national security advisor, who had also attended the information officers meeting, stressed that a Nevada atmospheric test should not be used as the first test merely for the purpose of shortening the time between announcement of test resumption and the first test, but he was not opposed to a first shot in Nevada if done for legitimate military and scientific purposes. When General Manager Luedecke forwarded some of these thoughts and AEC positions to Seaborg on February 10, he referred to a conversation between Seaborg and Bundy on February 8 when Bundy had given his opinion that a March 1 date for a balloon shot was not indicated and the AEC was not to proceed with the plans to meet this date. This conversation had served as a basis for the AEC's stopping preparations for an early atmospheric detonation in Nevada. Moreover, the AEC notified General Starbird that he should not speed up the opening date for Pacific tests before the previously agreed plan of April 1 (see Chapter IV). Luedecke now saw Bundy's statements in the information officers' meeting as somewhat conflicting with the conversation with Seaborg that had led to the AEC decision. Thus, in these few days, the opinion, certainly within AEC Headquarters, was that a March 1 balloon shot was not an option to be pursued, but there is no indication that this was conveyed to the field organization.

Apparently acting on the earlier information from Betts, Reeves, on February 13, authorized the Laboratory, contractor, and AEC personnel at the NTS to begin preparing immediately for an NTS balloon shot to be ready March 1. He noted the assumption that Area 7 would be utilized for this event, specified the two candidates, and told REECo to assess all problems related to meeting this date, including helium availability. Sandia was asked if they could meet the date, allowing sufficient time for practice flights which were not yet authorized. On the same day, Betts asked LASL to be ready for a possible balloon event on March 1. Two days later, Al Graves, head of the LASL Test Division, in messages to Jim Reeves and General Betts, noted that the balloon event was now called Musquash and would use the [redacted] Because of the desire for vulnerability measurements, LASL would consider moving the zero point and firing the shot at 500 feet altitude. Graves interpreted Betts' guidance to mean doing everything short of inflating the balloons. The next day, Paul Guthals, LASL Project Director for Air Sampling, expressed his feelings that there must be at least one day set aside for dry runs on the B-57 sampling aircraft operations since this was to be the first time sampling of this magnitude had been done for some time.

The uncertainty as to the possibility of opening with a balloon shot was further confused by a message from General Betts on February 17 which noted that the starting date for atmospheric testing was uncertain and might be any time during March or on April 1. He also expressed doubt that the low-yield device selected for the balloon event would satisfy the "nontechnical goals of the opening event." Thus, in a significant shift, Betts now planned to argue for a larger-yield balloon test at the NTS as the opening event and wanted the Labs to suggest candidates with yields larger than 10 kt that could be available on the necessary time scale. LASL dutifully answered two days later with four candidates. The first [redacted] and the second was the device tested as Fisher, which would be of interest in possible later studies of the effects of close tamping of earth on device performance in underground tests. LASL expressed dismay over the problems with the changing scenarios for the balloon test and the uncertainties in yield, date, device,

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etc., asked for any kind of definition possible, and noted that if the test were to be much later than March 15, certain diagnostics would necessarily be minimal because equipment and people would have departed for the Pacific. The point was made also that meeting the March 1 date was already a problem for the low-yield balloon shot since two weeks was the stated balloon practice flying time required before the detonation could be carried out, and practice flying was still prohibited.

On February 19, Milton Rex at NTS pointed out that a shot of yield more than 10 kt in Area 7 would necessitate lowering a considerable number of drill rigs in Areas 3 and 9, and this would obviously impact test preparations and recovery in those areas. He also posed the question of damage to the BREN tower in Area 4 if the yield went up and noted it appeared that the highest yield that would not be a hazard to the Linen high explosive stored in Area 2 was about 30 kt. Moving the balloon shot to Frenchman Flat (Area 5) would eliminate problems in Areas 3, 4, and 9, but would in itself have some other problems.

A message from Betts later that same day, February 19, did nothing to alleviate the uncertainty, stressing again the need to prepare for a low-yield balloon event, simultaneously plan for a higher-yield event, and also to be prepared to respond to a Presidential request, should it come, for an event sometime after early March. Foster gave a brisk Livermore reply to all this "what-iffing" on February 19, stating that this confused situation should be satisfied by the following: "Unless the situation can be better defined, I suggest a standard bomb of the appropriate political yield be taken from stockpile and dropped to take care of the political situation."

Schwartz informed Betts on February 21 that Sandia would continue preparations and would schedule a flight of the primary balloon for the low-yield event on February 26. Sandia would then be prepared for the detonation within 48 hours after the flight, or to meet a March 1 date. Any delays after February 26 would result in about a five-day lead time for detonation of the low-yield planned device and a longer lead time if a different device were selected.

Finally, on February 23, Betts canceled preparations for the small-yield balloon event and requested that LASL immediately prepare for a test of the device used in Fisher. His understanding was that the test might be executed within ten days after authorization, which would be requested from the President, and he reiterated that practice balloon flights were not authorized.

The situation was resolved on March 6 when General Betts told the Laboratories and the test organization to terminate all preparations for the Musquash event, expressing his thanks to all the staff for the work done in preparing this event. On March 7, Reeves, in a message to Betts, advised that preparations for Musquash readiness had been canceled.

Steady State vs. Breather

Testing had been proceeding long enough on the emergency, high-pressure, crash basis by early February 1962 that General Betts asked the Planning Board to consider the possibility of modifications to the underground testing program. He noted in February 7 message that although planning for underground tests after April 1 would go on, Chairman Seaborg had indicated to President Kennedy that the AEC was considering permitting the Labs and test organizations to have a reasonable "breather" after about April 1. An alternative approach mentioned, if practicable, would be based on a technical program plan and test organization which could safely and efficiently test on an extended "steady state basis." Betts suggested that perhaps the organizations were already approaching the latter course. Early in March he intended to submit to the Commission a program for the April 1 through June 30 period, and he wished to be in a position then to assure the Commissioners that "fatigue or overwork" would not create a hazardous testing situation. Thus, he asked for Planning Board comments and recommendations on the "breather and steady state alternatives" prior to March 1. Specifically, he wanted the Planning Board to compare operational efficiency, morale, safety, funding, atmospheric testing, etc. for each alternative. If the Planning Board recommended a breather as the preferable approach, Betts wanted their recommendations on the length of the breather and an estimate of the extent to which NTS operations would be curtailed during the non-testing period. The Planning Board addressed these questions, among others, in a meeting in Las Vegas on February 26.

The first Laboratory response giving thoughts on these alternative methods seems to be in a message from Jane Hall of LASL to Betts on February 16 proposing that the six LASL shots during the period April through June be performed in pairs spaced five days apart with each pair separated by about three weeks. LASL suggested that this method, which would allow test personnel to have breathers on a steady basis, was preferred to a long vacation followed by another intensive series.

The NTS Planning Board summary of their discussion on these alternatives was brief:

> It was agreed that the shot schedule . . . is feasible and necessary and serves the needs of the Laboratories from a technical standpoint. Any breather or further slowdown would complicate their problems. The "hot beds" vs. high paychecks problem still exists, but it was determined that this matter should be subject to separate study by OFO (Ed. note: ALOO Office of Field Operations) at the same time the drilling program is scrutinized. LASL and LRL proposed redundancy in crafts to reduce overtime.

Kenner Hertford of ALOO did not feel that continuing the present situation was at all acceptable. In a message to Betts three days later he made it very clear that in the light of discussions with testing organization personnel, observations of the operations in Nougat to date, and ten years of experience in the business, he was firmly convinced that:

> We collectively cannot continue the present pace for the next six months for many reasons. Excessive wages, in my opinion, are totally unjustified and can be the subject of not only extreme adverse national publicity, but also the subject of investigations either by the Congress or other government agencies. Both technical as well as supervisory personnel are gravely overworked with long hours and, in my opinion, cannot perform efficiently at this pace for another three to six months.

In addition, the support organization, including the contractors, was strained and while he was not too concerned about the probability of a nuclear accident, he was worried about industrial or high-explosive accidents. Finally he stated that:

> After discussing this matter here and carefully considering it myself, I strongly recommend that you recommend to the Commission and the President that we be instructed to go to a so-called steady state, effective not later than April 1 this year. This steady state would mean the following nuclear detonations: two each for LRL and LASL; one for either DOD, Plowshare, or the United Kingdom. This would mean a total of 5 nuclear detonations per month.

Clearly, this position impressed General Betts, who then asked the Laboratories to comment on Hertford's suggestions and his formula for the number of shots per year. Moreover, he asked them to submit revised schedules beginning in April in accordance with Hertford's proposed formula. Finally, and most importantly, he stated his opinion that "I believe that our underground program has reached the point where a steady state type of operations is appropriate. This has been my goal for a long time, and budget preparations for FY 1963 are geared to only one shot per week."

Bradbury replied on March 8 that the rate of two shots per month for LASL sounded reasonable if it could be assumed that testing would go on indefinitely. However, his main point was that there should be no restraints on NTS testing until the atmospheric testing was completed and that, in effect, the steady state testing at NTS should be postponed until after Dominic and become a policy for FY 1963 unless the political situation had changed. Bradbury said that, "In spite of the fact that people are working hard, we do not think that they are strained beyond ordinary physical endurance and that nothing should be allowed to interfere with any support that Dominic may require."

Taking into account the spirit of Bradbury's intent, General Betts provided modified guidance on the steady-state future in a message on March 13. He set forth a program showing more than five events per month for the period April through June and said that he saw that period as a transition time in going to a steady state situation. His guidelines included materially reduced overtime except in areas such as those directly related to supporting the Dominic program. He made a rather general point that in the future the Planning Board should assume responsibility for integrating budgets and budgetary limitations for weapons testing into the Planning Board's recommendations and proposed programs.

The details of these arguments are less important than the fact that the atmospheric testing program didn't end within the fiscal year, as had been expected. Moreover, the discussions of a reduced-level steady state and a reduced budget for the next fiscal year went on well into the summer.

In a message to Betts on June 8, Jim Reeves, newly appointed Manager of the newly-formed NVOO (Nevada Operations Office), summarized the status and actions related to a steady state for weapons testing at the Test Site. He began by reviewing the DMA guidelines (which had been discussed within the test organization in the spring) toward reaching standard workweeks of 40 hours, with exceptional cases requiring 48-hour workweeks. The consensus seemed to be that "The most logical approach to the steady state could be facilitated by establishing a basic workweek for support personnel at NTS of 45 hours (five 9-hour days) as soon as possible." The principal supporting contractors--REECo, H&N, and EG&G--had been asked to recommend an NTS standard workweek policy for a steady state. The recommendations indicated workweeks between 40 and 48 hours depending on the type of work, such as drilling, tunneling, personnel support activities, etc. After discussing the individual cases, Reeves directed establishment of: "an interim workweek for the Nevada Test Site of 45 hours, subject to the exceptions as indicated above as applying to EG&G, H&N, and REECo. Prior to January 1, 1963, the work load and workweek will be reviewed with the objective of reducing the approved workweek to 40 hours on or before that date. Any acceleration of the testing program beyond the eight events per month or 24 events per quarter which was used as a basis for the steady state study, or the introduction of unique or excessively complex experiments, will result in a general overall increase in the workweeks cited above or will require an increase in the personnel levels of the contractors work forces." His message ended with the suggestion that the employment level related to NTS weapons test work would remain relatively constant at about 6,200 employees in the period following a reduction in the basic workweek.

CHAPTER IV

RETURN TO ATMOSPHERIC TESTING--PACIFIC

The story of the return to testing in the Pacific in 1962 is one of many false starts, false directions, frustrations, and uncertainties. These came about because of the balance, or imbalance, of pressures among (1) a President who abhorred the thought of nuclear weapon testing in the atmosphere, who wanted to prevent the proliferation of nuclear weapon systems by the two large nuclear powers, and who felt that the initial step in so doing would be a test ban treaty, complete if possible, but if not, just atmospheric; and yet who also felt that as President he could not let the Russians advance beyond the United States in nuclear technology; a President, therefore, who was determined to give the Russians every chance to come to agreement, (2) a Joint Chiefs of Staff and an Atomic Energy Commission who respected the President's desires, but felt strongly that the Russians were gaining too fast by atmospheric testing, and believed that we could only maintain our lead by also testing in the atmosphere, (3) a State Department who never did know what they thought, (4) a Secretary of Defense who felt as strongly as the President that further nuclear weapon development should be prevented, but who also had to prevent the Russians from outdistancing us, (5) a split President's Science Advisory Committee, (6) a British ally which felt even stronger than the President that there should be no further proliferation, (7) a recalcitrant and "unreasonable" Russian opponent, (8) a technical organization whose morale had to be considered to a certain extent in the decisions made, (9) a set of nuclear weapons laboratory directors with strongly differing opinions on the need for future weapon development, and (10) a JCAE who felt that we should return to atmospheric testing as soon as possible. These conflicting pressures led to a period in which the President was grudgingly, dragging his heels all the way, taking those steps that led eventually to atmospheric testing, but in such a manner that the situation was never clear and was always changing as seen by the field test organization. The President continually kept alive his attempts to achieve a test ban treaty with the Russians and the British, and he finally attained that goal in 1963. It is quite clear that at any time in the interval between the resumption of testing in September of 1961 and the conclusion of the Limited Test Ban Treaty in 1963, President Kennedy would have signed a treaty with the Russians at a moment's notice, had that been possible.

The test organization, however that may be defined, loyally followed these fits and starts, even though occasionally a number of individuals in the system, both in the AEC and the DOD, clearly showed their irritation at this manner of conducting affairs.

Even when the President decided that atmospheric testing was necessary, he insisted that it be as little as possible, as few as possible, and as short as possible.

For some reason that is not quite clear, atmospheric testing was a special horror in Kennedy's mind, even though he himself apparently did not believe that long-range fallout would seriously endanger anyone's health. His actions seem to imply a feeling that underground testing would not seriously affect the international balance of nuclear forces, but that atmospheric testing would lead to sudden and large changes in our posture vis-a-vis the Russians. He was told that eventually we could probably test as much as a megaton underground.

As a result of these pressures, the AEC and the DOD, in the fall of 1961, while preparing for certain kinds of atmospheric tests, reversed their paths and turned the tests off, turned them on again, and put them in different areas. Even early in 1962 they were trying to decide where to do some portions of the testing and whether or not to do other portions. Still later, during the test operation, these pressures, plus those due to new and deeper thinking on the part of the weapon philosophers, led to continual changes and extensions. There were short periods of stability in which the test organization felt that it had a clear and agreed-upon plan it could carry out. But right up to the end of the operation, that feeling of stability was continually disrupted by the vagaries of nature or politics.

Program Formulation

Achievment of an agreed-upon program for the United States atmospheric series of 1962, eventually called Operation Dominic, was a long and arduous process of compromise between proponents in the AEC and Department of Defense, and opponents in Presidential circles, the Department of Defense, and the AEC. On the sidelines, pressures from Congress and the public affected things slightly.

For once, the Department of Defense got the jump in planning on the Atomic Energy Commission. While Operation Willow planning had officially been stopped during the moratorium, the concepts and reasoning were still clear in the minds of the DASA planners. Toward the end of the moratorium, the growth of AFSWC capability and interest in high-altitude effects specifically related to the antiballistic missile problem and the continued efforts in this field at Rand had led to the growth of a coherent recognition of the associated problems and a growing desire within the Air Force to do something about it. The Army was anxious to continue the development and testing of the Nike-Zeus antiballistic missile system, and the Navy had several stockpiled systems that needed testing. In July 1961 these pressures and the growing likelihood of test resumption led the DDR&E (Harold Brown) to ask Bill McMillan of Rand to lead a group to look at these questions. (Brown had little confidence in the ability of DASA, as then constituted, to handle the problem.) Within two weeks of its formation, he asked the McMillan Committee to consider the necessity for atmospheric effects tests and to recommend a possible program. That committee set about its work with vigor, and by the end of the moratorium had several meetings in coordination with DASA, AFSWC, Rand, the armed services, and certain representatives of the AEC laboratories. By the fall of 1961 they had convinced themselves, and apparently McNamara, of the necessity to conduct several high-altitude detonations.

The Navy, too, had seen the handwriting on the wall and in the last month or so of the moratorium had gone ahead on their own to outline and prepare for tests of the ASROC antisubmarine system and the Polaris fleet ballistic missile (FBM) system. They proposed to conduct the Polaris test on the Atlantic Missile Range, firing into a target area near Ascension Island. By September the Navy had some of the forces in place ready to respond immediately to any Presidential directive on the subject.

The Air Force had begun conceptual planning for an Atlas to be fired from Vandenberg Air Force Base to Kwajalein, to check out the Nike-Zeus antiballistic missile system installed there.

Thus, not long after the moratorium ended in late August the Department of Defense presented to the National Security Council and the President some of the arguments for returning to atmospheric testing, from their point of view. No decision was made.

Once the moratorium was broken and the President had ordered underground testing, the AEC began to move. In the early days of September, during Presidential discussions with Seaborg and McNamara concerning the appropriate US response to the Soviet resumption of testing, both atmospheric and underground testing were considered. McNamara asked Seaborg to determine what the AEC could do on a two-, four-, or six-week schedule to provide three shots of sufficiently high yield that they would be noted off-site and by foreign observers. Seaborg answered on September 5, the day that the President declared the resumption of US nuclear weapon testing, but his answer did not include any plan involving atmospheric testing. However, in preparing that answer Seaborg had had discussions with Luedecke and Betts concerning atmospheric test possibilities, and Betts, in turn, had discussed the question with the Laboratory directors and Kenner Hertford, Manager of ALOO. On September 7 Betts asked the Laboratories for their comments on the advantages and disadvantages of testing in the atmosphere, asking that they reply by September 11 so that a joint AEC-DOD position on atmospheric testing could be prepared for submission to the President in the near future. In his answer on September 8 Bradbury stressed the advantages of atmospheric testing, pointed out that things could be done appreciably more rapidly in that manner, and even went into detail on the possible time scales and possible sites that could be used. However, he did not urge an immediate return to atmospheric testing; quite the contrary:

> LASL has to ignore all the various aspects of propaganda factors one way or another. All the experts are in Washington. In general, we would recommend the following: Let us try out underground testing in Nevada as fast as we can and see what we can do and what troubles we do or do not get into. We may end up saying "fine, fine" or we may come running to Washington after a few months with a cry that we are not getting anywhere. I think we have to give it a good try fast. By the first of the year, we should have a pretty good idea of what the virtues and difficulties are. We will also have a better idea of where we are headed in the international situation. No big warheads really need testing today.

Kenner Hertford gave his opinion that one or two atmospheric tests could be staged quickly at the NTS without undue public reaction. Further correspondence between Betts and the Laboratories in those few days made it clear that Betts was reflecting a Washington feeling of anxiety concerning the slow rate of testing evident in our initial Nevada planning schedules. As it happened, it took ten days to get the first shot off after the directive to return to testing! Thus, the entire system was casting around to see what could be done in a short time.

General McCorkle of AFSWC was busy in the same circles in conjunction with Kenner Hertford, trying to determine what requirements might be put on the Air Force for the Nevada tests and for other longer-range possibilities. At their September 13, 1961, meeting the Nevada Test Site Planning Board concurred in an initial study concerning possible reopening of the Eniwetok Proving Ground and planned a September 21 meeting to develop an integrated approach to Eniwetok test requirements such as balloons, barges, cabling, etc. However it was triggered off, in about mid-September Hertford suggested to McCorkle that they study the possibility of a "quick and dirty" airdrop operation. They worked with Los Alamos and Sandia over the next day or so and by September 19 an initial concept of an airdrop air array operation was in hand. By September 25 Hertford was sufficiently confident of the concept to suggest to Betts that McCorkle be appointed Task Force Commander of an Air Force Task Force to carry out the operation. Betts told him to keep planning.

On September 21 Bradbury wrote to Betts suggesting consideration of "quick and dirty" airdrops of stockpile devices [redacted] Given proper authority, he felt that with about a week of preparation the Air Force could probably drop them and LASL could obtain minimum diagnostics (bhangmeter and radchem

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samples). He also noted that LASL and Sandia were already preparing MK 39 drop cases [redacted] The drop cases could probably be ready in about two months.

Thus was born the short-lived Operation Everready, a concept which contributed to the definition of an initial AEC atmospheric test program and to the beginning of the airdrop and air diagnostic capability used later in Dominic.

This groundwork led to presentation of an operational concept at the September 27 meeting of the Planning Board. [redacted] The initial readiness could be achieved using an AFSWC B-52, with minimum diagnostics in the B-52 and radiochemical sampling from a B-57. [redacted] At the same Planning Board meeting it was concluded that in something like two more months, because of the additional diagnostic capability that could be available by then, another three shots could be added. (Also discussed by that group was a concept for a Nike-Zeus high altitude test at Johnston Island, which the Board concluded could be done in something like six months.)

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During the next few days Washington picked up this airdrop proposal and, considering the priorities noted in the AEC Laboratory and Department of Defense messages of the last couple of weeks, made their own suggestions. On October 2 Gerry Johnson informed Seaborg that, "It is my opinion that such an operation would provide a means of conducting tests which are urgently needed, could be done very quickly, perhaps in a matter of weeks, and would provide some of the much needed information on devices which could be tested by this means." He recommended that the AEC, in coordination with DASA, quickly develop a test plan based on this concept and specifically suggested that proof tests [redacted] be considered for inclusion in such a series. On October 7 Betts, noting that there was a possibility of international pressures causing us to enter another moratorium very soon, requested that plans be developed for firing those three devices under the Everready concept by December 1. [redacted]

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In parallel with these actions the Laboratories discussed with DMA their broader aims for a longer-range atmospheric operation, but it was not yet possible to propose a specific list of devices.

The DOD, in the meantime, through the auspices of the McMillan Committee, DASA, AFSWC, and the armed services' representatives, listed those tests that seemed feasible in the moderately near future. Thus Gilpatric, in response to an NSC request and with Seaborg's concurrence, transmitted to the President on September 20 a joint AEC-DOD preliminary test program, and followed it up on October 9 with a more concise statement of needs. In addition to the Nevada program, that letter listed the initial five devices of Operation Everready as follows in Table XXIX.

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The systems tests proposed were two Polaris shots, the first of which could be ready in mid-December 1961; an Atlas-D (to be launched from Vandenberg), which could be ready within two weeks; and the ASROC surface-launched, solid-propellant-rocket, antisubmarine weapon, which also could be ready for full-scale tests within two weeks.

Seaborg followed up the next day (October 10) with a confirming letter in which he estimated the cost of the Everready shots to be perhaps $500,000 to $600,000 per event. Gilpatric requested approval to prepare for such tests at appropriate overseas locations, and Seaborg added that larger-yield proof tests and certain development tests could be carried out in a completely airborne operation within the next few months, but that it would take at least six months and a major operation to open up a Pacific test site for an extended development test program.

With the President's announcement on November 2, 1961, that he had directed the AEC and DOD to prepare for atmospheric nuclear weapons tests, both the AEC and DOD began to develop firmer test proposals. They were aware that the tests would be scrutinized individually, not only by the National Security Council, but by the President himself, and that real need for a test would have to be shown before the President would allow it. Their proposed tests also had to fit within the physical constraints of the possible test sites, which had not yet been chosen, and to a certain extent within the limits on total debris radioactivity release to be allowed. They were aware of the Presidential guidelines which were as follows: Tests will be conducted in the atmosphere only if:

1. the tests will provide information of substantial importance to the national defense,

2. the information needed can be obtained in no other way with reasonable time and effort,

3. atmospheric fallout is to be minimized in all practical ways, and

4. the military need for the tests outweighs the desirability of avoiding all atmospheric fallout.

Furthermore, the proposed tests should be ready within four months, that is, by March 1, 1962, and the tests should be conducted in the shortest possible time with the target of no more than three months duration. The National Security Council had tossed around the idea of limiting the release of radioactivity to that corresponding to 10 megatons of fission, but no hard and fast rule was made.

On November 5 Betts asked the Laboratory directors and Field Office managers to prepare a new set of test proposals for discussion at a meeting in Albuquerque on November 13, 1961. At that meeting Livermore presented 26 proposed shots and Los Alamos 15. Both Laboratories included devices in their listing that, in principle, could be tested underground in Nevada, explaining, however, that the programs would be delayed if they had to wait for the appropriate facilities in Nevada. Betts requested that the Laboratories confirm their proposals by TWX, which both Laboratories did on the 20th. LASL's confirming list was the same as presented in Albuquerque with the addition of two high-altitude shots, one of which might be combined with the proposed Department of Defense program. Except for a 1-megaton shot, the Livermore listing was the same as given in Albuquerque. It is perhaps worthwhile at this point to take a more detailed look at the specific proposals.

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Bradbury added, "We believe the country to have been badly mistaken in its belief as to the efficacy of underground testing and that the national policy which followed this opinion might have been quite different had the actual facts been known two years ago. We believe that we should find out by actual experiment what really can be done in space testing before we get trapped into one or another belief regarding it."

In mid-November, President Kennedy appointed a subcommittee of the National Security Council, to be chaired by Seaborg and consisting of Wiesner, Bundy, a State Department representative, and Gerry Johnson from the DOD. In preparation for a meeting of that committee, Betts sent to Seaborg on November 24 the DMA version of a proposed shot program taken from the above listing. DMA had reduced the list to 10 LASL shots and 14 LRL shots, including two high-altitude tests. He noted the possible conflict between the AEC proposed high-altitude tests and the DOD proposals.

On November 21 Gerry Johnson sent Seaborg the DOD proposal for three high-altitude shots; Starfish, Kingfish, and Bluegill. On the next day Gilpatric instructed the Chairman of the JCS to review the Department of Defense proposals for high-altitude shots and pointed out that 12 to 18 months would seem to be required if a meaningful three-event high altitude effects program were to be achieved. Consequently, Gilpatric noted that since only 4 to 5 months lead time were available, the JCS should plan to execute at least one, but no more than two, high-altitude effects tests. The Gilpatric memo apparently contradicted the Johnson memo.

Be that as it may, Seaborg sent to the President on November 29 the resultant National Security Council recommendation. Seaborg's letter noted specifically that the development tests would have to be done in the atmosphere because of the high cost and long development time of a deep space testing capability. [redacted]

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[redacted] He also noted:

> We have proposed and did plan that yields up to 100 kt would be conducted underground in tunnels; however, our experience with postshot contamination in the tunnels at the Nevada Test Site up to this time gives us some concern that our planning in that aspect was not realistic. It is now estimated that, if feasible at all, tests of this magnitude could be conducted at a rate of not more than a very few per year, even under favorable conditions, in a given tunnel complex. In contrast, large-yield devices can be tested in rapid succession in the atmosphere with relatively little advance preparation of the means of testing.

The letter went on to note that after appreciable discussion on the ground rules to be used and the philosophy behind the choices, the Subcommittee recommended for Los Alamos [redacted] was noted the intention was to test them underground in Nevada, but "In the event some or all of these cannot be accomplished satisfactorily underground, they may require inclusion in the atmospheric series." It was noted that the Laboratories would like to test the rest of the devices that had been on their list to DMA, but that the NSC was not recommending their inclusion in this operation.

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[redacted] These were the fore-runners of what became the Tightrope and Checkmate events of Operation Dominic, though these events were at other altitudes.

Seaborg added that the NSC recommended April 1, 1962, as a proposed readiness date for the series. It is fairly clear that this came about because the missile shots could not be ready until perhaps June, and the operation was restricted to a duration of three months.

Seaborg also noted the Los Alamos desire to try out a space testing capability [redacted] He proposed that the space testing might be accomplished on Starfish [redacted]

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The President clearly was not satisfied and requested that Seaborg decrease the number of tests by consolidating and substituting among the various proposals. Harold Brown also advised Seaborg that he disagreed with the AEC list, feeling that some six of the shots did not meet the criteria that:

> They can be fully justified on the basis of real (though not necessarily immediate) military importance, and there would be very great difficulty in performing them in other environments.

The President also sought other advice. In early December he discussed his concerns with Hans Bethe, who commented that while he did not consider atmospheric fallout very important, many people did, and as soon as we tested in the atmosphere our propaganda advantage would be lost. He felt that the United States should make a real effort to avoid atmospheric testing or at least restrict it to an absolute

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minimum. On December 7, responding to the President's request, Teller pointed out the surprises that we had already seen in the Soviets' progress to date, commented that we had to develop lightweight warheads for our rockets, and that we should work on high yield warheads that could do damage at high altitudes, hence reducing the effectiveness of the Russian missile defense system. He emphasized the importance of acquiring additional ABM information, both from a defensive and offensive viewpoint, and gave his opinion that the further development of clean explosives was very important since the recent tests had put the Soviets into a leading position in that subject. He further commented:

> Since we have not foreseen the present emergency and since we have not planned for it, the atmospheric testing program of the spring of 1962 will fall short of accomplishing the major proportions of the objectives stated above. It is nevertheless essential that we should proceed with an appropriate testing program next spring. The necessarily limited results of such a series will certainly enable us to plan a next series in 1963 in a much more fruitful manner. There is no theoretical way which can replace the hard facts obtained from experience.

Teller went on, "The plan which has been worked out by the Lab Directors and the DDR&E is the result of a careful study with which I agree." In addition, as the DOD had suggested, he urged that there be an ABM test as early as possible utilizing a missile launched from the U.S. to Kwajalein with a Nike-Zeus making the intercept at Kwajalein, and suggested that such a test might be done as early as May or June of 1962. He was optimistic about including in the high-altitude tests of the NSC plan some of the diagnostic apparatus which might be used to develop a space testing capability. He further requested that the President visit the Laboratories and perhaps make a public statement emphasizing that "the development of nuclear explosives can be used to provide us with the strength that ensures peace." Teller also made it clear that he was looking ahead to a test series in 1963 when adequate instrumentation could be available to achieve the most important objectives.

Edward's comment on the value of the operation to be done in 1963 must have struck the President in an odd manner, since he was apparently worried at the time, not so much about the round of Soviet tests that were now almost finished, but about the rounds that might follow in 1962 or 1963, and he could just see both countries continuing the escalation.

On December 19 Conrad Longmire (LASL), in discussions with Panofsky on the subject of atmospheric testing, pointed out that the Russians were apparently doing both systems and effects tests, emphasizing his belief that it was very important for the U.S. to undertake such testing lest the Soviets gain significant advantages, especially in the critically important phenomena of EMP and radar and communications effects.

On January 8, 1962, Betts informed the Laboratories:

> With respect to an item to be added to the Presidential-approved list, a mechanism has been worked out whereby this type of change is accommodated. In brief, the Chairman approves the inclusion of the event and notifies the White House Staff of the added event. The Chairman's notification contains three essential elements as follows: A. the tentative firing date; B. purpose of the experiment; C. the reason why this particular experiment was not included in the basic list approved by the President. Therefore, on each additional event for which you request authority to execute, please advise me as early as possible, repeat, as early as possible, of these three inputs. Once you have provided me in a timely fashion these three inputs, you may assume that authority will be forthcoming prior to your anticipated execution date. Therefore, you may take all steps necessary short of the actual firing prior to your final authorisation for the specific shot. These steps may be taken without recourse to DMA.

After the November 29, 1961, meeting of the NSC, the Laboratory programs changed very little. There was a continual review on the subject between Betts, Foster, and Bradbury with comparatively minor changes coming about, but there was no further NSC review until after the President had decided to test.

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5 U.S.C. 552 (b)(3)
DOE, EXEMPTION 3

On February 27 Foster replied for himself and Bradbury that their careful review of the proposed devices showed no overlap in the technical objectives and affirmed mutual interest in every experiment listed; therefore, the Laboratories would prefer to keep all six. [redacted] Thus, there was a continuing round of conversations on the subject among Harold Brown (DDR&E), JCS, Seaborg, and others. However, when word got around that as part of the Everready practice runs at Tonopah, a dummy TX-43 containing only high explosives had gone off 3,000 feet underneath the drop aircraft instead of 3,000 feet above the ground and that no explanation could be found for the misadventure, the enthusiasm for testing such devices quickly waned in the JCS. With the concurrence of Harold Brown, the Navy and the Air Force kept the Atlas and the Polaris systems tests alive. Apparently, for several months early in 1962, tentative program changes were being made at the level of Seaborg and Brown, changes which evoked continuing admonition to the sponsors that the President had not approved the specific changes and had not agreed to atmospheric testing.

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5 U.S.C. 552 (b)(1)
NSC Ex 1

A couple of other program proposals should be noted. Khrushchev had threatened us with a 100-megaton bomb in July 1961. Apparently, in reaction to that, a number of people in the U.S. began to puzzle about such a device. On September 7 Betts asked Foster and Bradbury for detailed estimates for a possible 100-megaton weapon.

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5 U.S.C. 552 (b)(3)
DOE, Ex. 3

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5 U.S.C. 552 (b) (3), Ex. 3
5 U.S.C. 552 (b) (4) Ex. 4

DOE

[redacted] commented that he had not analyzed these sugges- [redacted] agreed to send a memo to the President which would include the probable effects of a 50-megaton detonation at various altitudes, the time required for U.S. development for such a device, and the possible military uses of a 50-megaton bomb. The Chairman had spoken to the President earlier in the day concerning the 50-megaton atmospheric detonation that had just been announced by the Soviet Union.

Following the Commission meeting, Betts promptly started conversations with the Laboratories on the feasibility of early construction and tests of such a device by the United States, asking for comments by October 27. Sewell pointed out the delivery difficulty for testing if it were to be an airdrop, but Sandia stated on October 26 that a B-52 airdrop was feasible with a parachute they were presently developing. On October 27 the U.N. General Assembly asked the U.S.S.R. to "refrain from carrying out their intention to explode in the atmosphere a 50-megaton bomb," and on the same day Seaborg sent to the President a letter commenting that the General Advisory Committee was convinced that the AEC could, within a short time after a Presidential directive, come up with a single weapon having a yield of about 50 to 100 megatons. The President apparently immediately said to forget it. On October 27 Luedecke wrote "No further action required" on one of the messages on the subject. On October 30 the Soviets exploded their 50-megaton device. It went a little large.

During the moratorium, the last year or so of planning on the part of the AEC and DOD had assumed that any further weapons tests would either be underground or in deep space. The AEC, therefore, as noted earlier, tried to get a shot into the Dominic series to test the deep space concept and gain experience that would be valuable if that method of testing became necessary in the future.

In response to Betts' request on September 25 for suggested actions in atmospheric and high-altitude testing, Sandia prepared a detailed plan which would use a Thor missile launched from Johnston Island to carry test devices to altitude. The Sandia plan estimated that 6 to 10 months would be needed to prepare for detonation of a large yield device at altitudes between 100 and 300 kilometers. The plan listed

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the objectives of high-altitude tests in the following way:

> A United States capability for testing nuclear devices outside the earth's sensible atmosphere should be established to provide: (1) A capability of testing large-yield weapons with reduced fallout in the event surface testing is not authorized. (2) A capability of testing large-yield weapons with reduced fallout in the event unacceptable atmospheric contamination is reached by United States and/or Russian surface tests. (3) A capability for vulnerability testing of complete reentry vehicle warhead systems in a vacuum or at appropriate intercept altitudes under controlled conditions. (4) A capability for conducting effects and phenomenology experiments on nuclear devices detonated outside the earth's atmosphere.

The report of the Sandia plan was distributed on October 2, but Don Shuster of Sandia was impatient: he notified the Planning Board members and the Laboratories that Sandia had studied ways to accelerate development of a high-altitude test capability and suggested that the Sandia work on a 300-kilometer Thor system might be applicable. Bradbury replied on October 16, 1961:

> The present NTS underground operation allows somewhat inadequate testing of small devices, but may never allow testing of megaton or larger weapons. Your proposal fits the spirit of the present limitations, i.e., no atmospheric contamination. Therefore, it seems to me that Washington should in the near future allow atmospheric testing or should approve an operation in deep space as you propose.

On October 19 Bradbury urged Betts to consider Shuster's proposal as soon as possible and warned against again going into a moratorium ignorant of different ways of testing. As noted before, Bradbury, in his November 20 message to Betts, proposed that there be a test at an altitude as high as could conveniently be reached in order to assess what really could be done in space. The proposal was discussed at the November 29 meeting of the National Security Council, and it was decided that "Consistent with satisfying DOD requirements, considerations will be given to accommodating the AEC desires to develop a capability for space testing in connection with the proposed 400-kilometer DOD experiment." (This effort led to the Starfish test.) By December 1, 1961, LASL had done enough work to respond to the NSC suggestion. Froman informed Betts that neither of the high-altitude shots, Starfish and Bluegill, would be fully satisfactory for learning how to test warheads in space, and added:

> In particular, 1.5 megatons at 400 kilometers is so large and so close as to cause serious saturation problems in detectors suitable for diagnostics in space testing. We believe that we should check out methods of space testing in order to avoid ignorance in that field such as we had about underground testing. We therefore propose that the AEC sponsor a shot [redacted] The best choice of yield requires further calculations and considerations. Incremental costs will be small because we shall instrument and man to do all we can on the 400-kilometer shot in any case. Additional merits of such a test include Argus effects the DOD originally planned and data relevant to Vela Hotel, Vela Sierra, and physics. Effort would also be made to follow fission products and get a handle on fallout from this altitude.

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On December 21 LRL offered support for the 1,000-kilometer test. Foster, in a message to Betts, said:

> It is LRL's understanding that at the present time, there are two high-altitude shots planned by the DOD from Johnston Is[redacted] techniques. [redacted] purpose. The second one, 1.5 megatons at 400 kilometers, we hope to use to test diagnostic methods. A second shot above 400 kilometers would be desirable for our purposes [redacted]

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Within LASL, Hoerlin and Taschek began to press very hard for the experiment. On December 26 they suggested to Bradbury that the shot should be at 1,500 kilometers or higher. In a January 2, 1962, message Bradbury requested that Betts formally advise the Commission of the LASL desire to add to the proposed atmospheric test program an additional high-altitude experimental shot whose primary purpose would be a proof test of weapons diagnostic systems applicable to possible free space testing beyond the region of appreciable geophysical field coupling. His message gave some details of the LASL concept:

> Specifically, the proposed experimental shot would be conducted at Johnston Island using the same type of vehicle, presumably Thor, presently proposed for two DOD high-altitude effects shots. The planned altitude of detonation would be between 1,000 kilometers and 2,000 kilometers (the maximum Thor could reach). The nuclear warhead would be [redacted] whose yield would already have been measured in an earlier part of the proposed series. Thus, the diagnostic techniques may be tested against a reasonably typical two-stage thermonuclear weapon of known yield.

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Bradbury went on to comment:

> We also recognise that there may be some inevitable delay in determining the response to this proposal at the ultimate level of authority required. Since, however, delay would now preclude the inclusion of the experiment at all and be fatal to its success as well, we are proceeding with technical planning and preparation in the hope that it will eventually be found possible to approve formally this additional experiment.

Bradbury bolstered his case at the JCAE hearings on January 18-19, noting:

> The Laboratory is proposing a very high-altitude shot (Urraca) at 1,000 or 2,000 kilometers using a device of known yield as an experimental test of the diagnostic techniques of in-space testing should this ever become necessary or authorized. Initial studies indicate that this could be a quite powerful and not exceptionally difficult method of weapon testing to be carried out with warheads of reasonable weight and not too great distances from the earth.

Betts continued to avoid the issue. On January 18, 1962, he sent a message to the Laboratories asking for a review of the atmospheric test program and giving his understanding of the situation, but he did not mention the deep space shot. Sandia forced the issue by requesting two reentry vehicles and telemetry antennas from the Air Force, one of which was for the deep space shot now named Urraca. This request brought up the question of funding and led to a January 26 message from the Chief of DASA to DMA, which pointed out that since the shot was not yet approved the AEC should guarantee the funding for such equipment.

On February 6 Bradbury offered to give up the land-based measurement of the angular distribution of prompt radiation from the [redacted] if it would help win the argument to get the very high-altitude shot. He commented that we could probably make a stab at getting the neutron spectrum on that shot.

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By February 12 Betts had transmitted the LASL request for Urraca to Seaborg and Seaborg had sent it to the President, since on that day, Seaborg noted to McGeorge Bundy, "I have been informed that the DOD has run into problems in funding the high-altitude (1,000 to 2,000 kilometers) shot." However, by way of further confusion, on February 13 Betts authorized the Labs to make complete preparations for Urraca, noting that it had been approved by both AEC and DOD, and, hence, was likely to be approved when it was submitted to the President. There was one more hurdle to jump. Noting that Urraca had been added to the series, CJTF-8 requested an additional Thor since the Department of Defense felt that the five Thors previously requested should be reserved for Starfish and Bluegill and, of course, they still had Kingfish in their minds. At the Commission meeting on March 7 (after the President had announced his intention to proceed), Gerry Johnson pointed out that there was a Urraca funding problem and asked if the AEC could purchase the $1,700,000 Thor rocket required. After some further negotiation, the AEC agreed to pay for the missile. Thus, Urraca became part of the intended Dominic series.

DOE Ex. 1:3

Another stir in the proposed program arose from LASL discussions with the Army concerning the Nike-Zeus warhead, which was to be the [redacted] Thus, in his November 20, 1961, message to Betts outlining the proposed LASL programs, Bradbury also recommended a high-altitude test of the [redacted] at an altitude of 125 to 150 kilometers (which was about the intended use altitude of the Nike-Zeus) in order to make detailed output measurements. The shot was discussed by the NSC at their November 29, 1961, meeting during which it was concluded that the [redacted] experiment (Bluegill) should partially satisfy the AEC interest in obtaining AICBM effects data at operational altitudes of Nike-Zeus. In studying the problem of how to measure the angular distribution, which was important because it entered into the question of whether the Nike-Zeus warhead had to be stabilized or not, the LASL test division had concluded that a better measurement of the angular distribution could be made using a ground-surface shot with appropriate instrumentation placed around the device. The problem of measurement of the angular distribution at high altitude would be very difficult because of the necessary placement of many detectors at appreciable distances around the bomb. On the other hand, the spectrum could be measured quite well at high altitude by time-of-flight techniques, although LASL also intended to measure it on the ground shot. Thus, on November 30 LASL requested that the Task Force begin searching for an island on which to do the ground test.

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Apparently the DOD had not been aware at the beginning of the November 29 NSC meeting that LASL would propose tests of the [redacted] warhead at high altitude. Introduction of the LASL proposal at that meeting caused some confusion, possibly because the DOD-proposed Bluegill [redacted] kilometers. [redacted]

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> **In view of the confusion caused at our recent meeting by the sudden injection of a high-altitude effects shot sponsored by LASL, I feel it might be useful to discuss with you the DOD procedures and requirements in this area. As you are no doubt aware, there is a gray area in responsibility for weapons effects measurements between the Department of Defense and the Atomic Energy Commission. However, the division of responsibility has been, of course, to develop weapons and to make those diagnostic measurements that affect the performance and design of the weapon. On the other hand, the DOD responsibility has been to measure those outputs and the effects caused by them that are of military interest.**

Brown suggested that LASL may have made the suggestion because of a request to make a specific effects measurement which may have been transmitted directly from the Army to LASL. Brown pointed out that a request from the Army does not constitute a requirement from the Department of Defense, and added that DASA, for the Department of Defense, had the responsibility of assembling and evaluating the services' requirements for weapons effects information. Thus, if any AEC organizations had an interest in a specific measurement, they should discuss that with DASA rather than with a military service. He commented that at the present the DOD did not feel the need for these measurements strongly enough to consider it as justification for a shot. Betts then suggested to Bradbury that the subject of output measurement

DOE Ex. 1:3

responsibility be addressed by LASL. Bradbury's response was in strong disagreement with Harold Brown's letter:

> It has always been previously assumed by LASL and, we believe, by the AEC that it was the responsibility of the AEC to develop weapons and make appropriate diagnostic and experimental measurements toward the subject and, in addition, provide, by calculation or direct measurement, the using agency with definitive information regarding the fundamental output of bombs. Specifically, when information on the x-ray or neutron output of bombs is required by the DOD, it is assumed to be the AEC's responsibility to furnish it.

He went on to quote many examples on both sides of the picture stating, for example, the effects of nuclear weapons on nuclear weapons were an AEC responsibility as was the determination of the actual emanations from a detonating nuclear system. Bradbury commented that LASL now intended to get the spectrum from measurements on the ground; if the ground shot was not practical, we would get what we could in an airdrop or possibly from a ship. He also commented that he did not disagree with the recent verbal statement by DDR&E that no one really needed this information as precisely as many of the subordinate agencies seemed to want it. He denied that LASL had made the proposal at the request of the Army and said, "If he will call off his dogs, we will agree not to go overboard in detailed spectrum diagnostics." The next day, Foster agreed with Bradbury's viewpoint, stating:

> Specifically, my understanding is that the AEC has a responsibility for warhead design and output including blast, x-rays, neutrons, and gamma rays. The 'gray area' is the transmission of these effects. The DOD responsibility is the response of military equipment and personnel to these effects. This position seems adequately covered in Starbird's letter to Dr. Brown, Director of LRL, on November 23, 1960.

Thus, the proposal for a 150-km, [redacted] was withdrawn by LASL, and preparations for a surface shot to measure the [redacted] continued for the next month and a half. But the pressure led to continued reduction in the project, and Brown was still unhappy about the idea of the AEC making such a measurement.

At the same time, LASL continued its arguments for Urraca, which was also bothering the DOD. Finally, Bradbury gave a bit, commenting on February 6, 1962, that:

> If it would help to strengthen the support for the 1,000-kilometer shot, we could probably say we could make fair neutron output measurements on this shot and thus abandon the need for a ground-base[redacted] We would still obviously need an airdrop yield shot of this device. In other words, if something has to give, give on the Baker Island shot, making it into an airdrop, but save the high-altitude shot if at all possible.

Withheld Under 5 U.S.C. 552(b)(3)
Exemption 3

Betts quickly took him up on that suggestion and the surface shot was canceled.

One other small flap in planning the series should be noted. On January 24, 1962, Betts informed the Laboratories that the Department of Defense had asked (a) what was the maximum yield warhead possible at the weight of the present Titan II reentry vehicle and (b) could either Laboratory have such a test warhead available for the Dominic series? Further information about the DOD concept was also provided: the Titan II would be the booster, the detonation would occur at an altitude high enough to avoid aerodynamic reentry loads, and consequently, the nose cone was required mainly for warhead protection during launch and exit from the atmosphere. Betts requested that ALOO forward a coordinated Laboratory reply within two days. On January 27 Glen Fowler of Sandia provided the following response:

> The commitments which have already been made by the weapons laboratories for the Dominic operation are such that the idea of a special Titan II test on the same time scale is greeted with a somewhat low level of enthusiasm. However, if this program must be undertaken for good reason, there is only one approach that is practical, namely, to use the Mark VI RV/XW-53 warhead combination now being designed for the Titan II. To do this, LASL would have to provide a nuclear system and Sandia would have to provide a complement of warhead hardware as well as a special fusing system for the test. A command-enable timer or a direct command firing system similar to that being built for the Dominic Thor would be required. We estimate that this would add approximately 100 pounds to the standard RV weight of 7,564 pounds. With command firing, detonation altitudes would be limited by reentry ionization problems and could occur down to 200,000 feet. At or above this altitude, neither reentry deceleration nor temperatures would present a problem. Flash blindness consideration, however, would suggest that the detonation altitude should be much higher, like 400,000 feet. We believe that the necessary nonnuclear hardware could be built in time for a July flight, but consider that it would be likely to affect current effort on the XW-53 warhead development to some extent and also would be likely to cause some difficulties with the preparation for the Dominic Thor operations. The exact effect cannot be assessed without a more detailed study. The LASL components probably could be made available for a July flight date with similar program interference problems. All of the above statements apply to the use of the presently planned XW-53/Mark VI RV payload for the Titan II, [redacted] Considerable design, test, and fabrication effort would be required and there is little doubt that such an effort would cause interference with both weapons development and Dominic effort. In Dominic time scales, the only available launching site appears to be at the Atlantic Missile Range.

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There was no further discussion of the Titan suggestion. The program for Dominic was firm enough to issue the listing and schedule shown in Table XXX.

On March 2 the President publicly announced his decision to resume atmospheric testing, giving the readiness date as April 23, 1962. By mid-March, in response to Betts' request for additional test needs that might have come as a result of tests in Nevada, an Atlas system test and a Polaris system test were included in the schedule.

On April 11, 1962, the Commission met and approved the proposed nuclear test program, specifically including the Atlas and Polaris tests. However, L. K. Olsen, the Commissioner who had not been present at that meeting, noted that in reading over the staff paper he did not find proper justification for the shots, especially the Polaris and Atlas shots, which were justified only by reference to two letters which were not themselves part of the staff paper. On April 12 Seaborg forwarded the list to the President along with a note pointing out that (a) it should be considered a flexible list, (b) that the operation might extend into July, (c) that the Commission had not yet reviewed the operational aspects of the Atlas, and (d) that there was a finite probability of a malfunction which could lead to one or more of the missiles being destroyed in flight, probably burying special nuclear materials deep at sea.

On April 24 Starbird received from Betts a message beginning, "This message constitutes the authority to conduct the atmospheric nuclear test program as follows: On behalf of the Chairman, AEC, I am advising you that the President has approved execution of the atmospheric test program." The program referred to at the beginning of Dominic was as shown in Table XXXI.

Digression on Test Methods

One of the main reasons we were able to go back to testing, both underground and in the atmosphere in 1961 and 1962, was the vast experience of the technical

TABLE XXX
DOMINIC SCHEDULE
February 20, 1962

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5 U.S.C. 552 (b) (3)
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personnel. The point should be made that their training and experience had given them a broad and deep understanding of the problems that had to be solved. The understanding required in development of the techniques that had been used in atmospheric testing was still fresh in their minds and that understanding could be used to develop new techniques in a new, higher altitude medium.

Specifically, the type of testing that we did underground, immediately after the moratorium in the fall of 1961 and the spring of 1962, was not a type of testing that had been done before or with which we had any particular experience. Los Alamos had fired three underground shots before 1961, that is, Jangle, which was buried at 17 feet, and Pascal A and B in Hardtack Phase II, 1958, which were a few hundred feet underground. In these tests the diagnostics were not particularly critical to the experiment. Certainly in the case of the Pascals--the two Pascal shots came roaring out of the holes like a rocket and allowed normal radiochemical sampling with airplanes in the cloud--the actual results were so far different from expectation that the diagnostics were not critical. To illustrate this point in more detail, the basic measurements made on development shots fired in the atmosphere in Nevada were the reaction rate as a function of time, the yield of the bomb as determined by the fireball expansion, and those data that could be derived from radiochemical analysis of samples collected from aircraft fitted with sampling tanks and flown through the debris cloud at fairly late times. The range of intensities to be dealt with in

TABLE XXXI
DOMINIC SCHEDULE

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measuring the reaction history is very great, from essentially as low an intensity as one can measure with very sensitive detectors to the peak of the intensity curve, a range, in some cases, of as much as 10^{30}. In earlier atmospheric testing that range of coverage was accomplished by using detectors of similar sensitivity placed at several distances from the bomb. A common array, for example, might have 10 to 15 detectors (or even more) with the closest detectors in a tower or a balloon cab essentially right up against the bomb and the most remote detectors perhaps 1,000 or 1,500 yards away. Signals from these detectors were then run through very fast cable to recording stations for enough away to survive the blast, say 1,500 to 2,000 yards from the bomb. For the underground tests, because of the containment requirements, the same experimental philosophy could not be used. The close-in detectors could be placed essentially as in the past, as long as the signal cables were placed approximately along a radius from the bomb. Initially the maximum distance of a detector was limited by the size of the largest usable canister to about 40 ft. This limitation led to the use of insensitive detectors and great amounts of shielding, which, in turn, led to great cable troubles because the cable itself becomes a detector at not very much higher radiation level than the intended detector itself. Thus, the initial underground shots often failed to give records of the high-level gamma data, and it was some time, perhaps a year or so, before new systems were developed to get all the desired data. (Initially scatterers were used; the eventual solution involved precisely the physical effects that caused problems in the early underground tests -- in the so-called Compton diodes invented by L. K. Neher of LASL.)

There was a similar problem with radiochemistry. In atmospheric testing we depended upon "complete mixing" in the cloud, which meant the cloud was allowed to turn over and mix within itself for some time, an hour or so, before sampling was attempted. When sampling was attempted samples were taken from various parts of the cloud in order to ensure they were representative. In underground tests, this process could no longer be used. Whether any sample obtained from the melted and resolidified pool of rock would be representative was a serious question. Initial attempts to allow radioactive gas to flow through small pipes to a gas collection system on the surface were not very successful because of strong fractionation in the piping system, if any sample at all was obtained. Again, the initial results were most unsatisfactory, and it was only after appreciable experience in drilling, sampling, and treating the samples appropriately that satisfactory results were obtained.

Similarly, when the possibility of returning to atmospheric testing arose in the fall of 1961, it became clear almost immediately that this would not be the atmospheric testing of old. Testing at Eniwetok and Bikini from 1954 to 1958 had resulted in the development of well-defined procedures. Diagnostics for most devices fired at those sites in that period consisted of reaction history, yield, and whatever data on the internal workings that could be inferred from radiochemistry. Reaction history was obtained in a manner similar to that described for Nevada, using detectors located at several distances from the device which was placed occasionally on towers or on the ground, but most often on barges, at some distance from a land recording station. The use of barges allowed firing of quite large shots without producing large craters and without permanently disturbing the islands. About one out of eight shots was thoroughly diagnosed, including attempts at real-time determinations of some details of the workings inside the so-called hydrogen bombs. These attempts, in general, involved complex arrays of instrumentation on small islands, with high-speed cables running into detector systems some distance away.

In the fall of 1961, when we were told to prepare for testing, the political ground rules imposed quickly made it clear that the earlier techniques could not be used, at least not in the earlier form. Even the testing location was not known, although the most likely site seemed to be open ocean somewhere south of Hawaii. The method of bomb emplacement had not been settled; whether by airdrop, which meant very careful packaging of experimental devices to be handled in that manner, or by emplacing the devices on ships that could be destroyed. Furthermore, it was politically clear that we would be testing only devices of fairly large-yield, at least in design, and the smaller tests would be done in Nevada. Therefore, from a diagnostic point of view on the AEC tests, it was accepted very early on that we would attempt to measure only the yield and a very small portion of the reaction history, as well as, once again, obtaining what data we could by inference from radiochemical samples. When preparations for Project Everready began it had been assumed that bhangmeters would be used to measure yield. However, it was also known that there were essentially no bhangmeter calibration data from large bombs fired in the air far above ground level: even the calibrations we had were suspect. Thus, an early requirement

5 U.S.C. 552(b)(3)
DOE Exemption 3

was measurement of the fireball growth rate. We had up to this time fired only two large-yield devices by airdrop: one, the 500 kiloton King shot on Operation Ivy and the other the [redacted]egaton Cherokee shot of Operation Redwing. In both cases, we had island stations at which to place fireball cameras, even though this system did not work on the Cherokee shot because the bomb was dropped out of the field of view of the cameras. A flexible system was required, that is, one with some kind of tracking, if possible, or at least some way to tell the bomb position at the time of detonation. An essential requirement of the fireball method is to know the distance from the camera to the detonation point in order to determine accurately the diameter of the fireball. Because the yield is proportional to the fifth power of the fireball diameter, if a yield measurement accurate to within 5% is desired, the diameter (and hence, the distance) must be accurate to within 1%. Thus, it is immediately clear that if the distance cannot be measured to better than 4 or 5%, then the fireball method of yield determination is not much better than bhangmeters or anything else.

Since this problem was recognized at the inception of the Everready program, plans had been made for installation of fireball cameras in C-130s and development of distance measuring equipment (DME) had begun. However, it was fairly clear to the people responsible for this measurement, primarily Art Cox of LASL, that these were very unreliable systems. This need for accurate distance determination was one of the critical items in the technical judgment that it was necessary to move to Christmas Island. Moving to Christmas Island would provide a stable base for proven radar equipment capable of tracking beacons placed on the drop device as well as in the bomber. From these instruments it was possible to have the cameras pointing at the bomb at the time it went off and measure the distance with sufficient accuracy to provide the desired accuracy in the yield measurement. Once again, this problem was comparatively new to the testers. In earlier operations using either barges or towers, the distance measurement could be inferred from surveying data acquired before the test: for airdrops in Nevada there were sufficient camera stations at well-known points that ordinary triangulation could give the distance with required accuracy. But doing this measurement from floating or airborne platforms in the middle of the South Pacific was an entirely different problem.

For measuring the small piece of the reaction history required for the atmospheric portion of Dominic, there was plenty of experience on previous operations. However, for those operations the measurements were made from roomy ground-based stations, for which Malik, Wouters, Partridge, and Theobald had developed quite satisfactory systems capable of successful measurements from appreciable distances. Installation of these systems in aircraft or on the decks of ships led to design changes imposed by limitations of tracking, antenna space, and electrical power. Thus, the systems that were put into the Everready C-130s to make this measurement were questionable and untried. Consequently, again from an experimental point of view it was preferable to go to Christmas Island and use dependable ground-based systems while learning how to make airborne systems dependable.

There is a fundamental point of AEC nuclear weapons testing that should be made. The experiment is the device; the experiment is something that is being conducted by the developmental portion of the Laboratory. The field forces are making measurements; they are not doing experiments. Thus, they seek high reliability in making those measurements and if they wish to try some new method of making an old measurement, then that new method is usually done in parallel with an old method and done many times until the bugs can be worked out and the new method becomes as reliable as the old one. This philosophy is pertinent to the development of nuclear weapons, but it is a somewhat different philosophy than that which applies when atmospheric effects measurements are to be carried out for atmospheric explosions. For example, one set of instrumentation, and one site properly instrumented, can be used for detonation-after-detonation for development shots, where the object is to assess the device performance. On the other hand, if one wishes to conduct effects tests, such as the high-altitude shots in Dominic, then it may be necessary to change the basic instrumentation from one test to the next. It is certainly necessary to design the measurement specifically for a particular shot, since in an effects test there should be no question about the device performance. The question is, rather, what are the effects of that explosion on the ambient environment: it is the effects on the environment that are to be measured, not the inner workings of the device.

Early Preparations

The beginnings of Project Everready have been discussed in the program section. However, other moves toward atmospheric testing also took place very early in September 1961. The discussions between President Kennedy, Seaborg, and McNamara in the first days of September clearly raised again the possibility of atmospheric testing. As a result, both the AEC and the Department of Defense began to make arrangements for that possibility should the President wish to move in that direction. In addition to high-level consideration of this possibility, it is also clear that many people in both the AEC and DOD began such moves because of internal convictions or desires to solve specific problems, and, in some cases, just to prevent embarrassment later.

On the AEC side, as noted before, Reeves, as a result of the September 13 Planning Board meeting, had authorized H&N to determine the status of the Eniwetok Proving Ground, and they were encouraged to send a small survey party there as soon as permission could be obtained from Washington.

At the same Planning Board meeting the LASL-proposed high-altitude Nike-Zeus warhead test was mentioned, and Johnston Island became a consideration. By September 29, 1961, Sandia had prepared a report on the possibilities of high-altitude testing which included delivery systems, possible launch sites, modifications of reentry vehicles, fusing and firing systems, diagnostic techniques, and safety. The report recommended the use of a Thor missile launched from Johnston Island, outlined a complete fusing system, including warhead destruct circuits, and suggested a tracking system. The report also included drawings showing the [redacted] mounted in the modified Thor reentry vehicle. Sandia estimated that the first shot could be ready in six months.

The effort that was to become Project Everready led to the development of many tools which were used later in Operation Dominic. As early as September 20, 1961, LASL asked Sandia to prepare some device drop cases, and by September 29 Sandia had developed a plan to provide (a) a universal test vehicle capable of carrying any of the current untested warheads and (b) the necessary instrumentation and support to obtain reasonable diagnostic information. They proposed that airdrops at the Eniwetok Proving Ground be made from a B-47 or B-52 aircraft instrumented with radars and fireball cameras. The Mark 39, Mod 1, Type 3 (Trainer) bomb case would be adapted to the [redacted] or other warheads. Appropriate fusing was arranged to preclude ground burst. This system, Sandia said, could be ready for its first test in two months. Other equipment soon appeared. While the first concepts of Project Everready involved the simple drop of a stockpile device off the west coast, discussions in late September quickly solidified into an operational plan that might give the Laboratories at least minimal diagnostics. In this plan the B-52 itself would be equipped with a fireball camera and a bhangmeter to measure yield. Initially one, but later two, C-130 aircraft would be equipped with optical and electromagnetic gear

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to measure the interstage times, and later they would also have fireball cameras. Sampling support would be provided by the B-57s now assigned to Air Weather Service. The shots could be located either at Eniwetok atoll or over the open ocean near Johnston Island or Hawaii.

On October 7, 1961, Betts told the Laboratories to be ready to conduct three airdrop tests by December 1, and a mad scramble began in the Laboratories and at AFSWP. The plans group of AFSWP sent McCorkle (AFSWC) an initial concept on October 8:

> The ground rules are that the series would be of relatively short duration with deployment to Hawaii within 10 days after authorization. The concept of operations is for four war-reserve weapons to be dropped from a B-52 aircraft under the control of AFSWP with the detonations to be over the open seas approximately 350 nautical miles southeast of Hilo. Three days of practice missions will precede the first drop which will be done by an airborne Task Force consisting of a drop aircraft, airborne diagnostic instrumentation, photo, command and control, weather reconnaissance, and air-sea rescue aircraft. The Task Group Headquarters will be at Hickam, with all aircraft staged out of the Hawaiian area. The command and control or AOC aircraft will be an RC-121 and a possibility for the diagnostic aircraft is an instrumented C-130 from AFCRL.

Also mentioned was the possibility of getting C-130s from the Air Photographic and Charting Service. By October 9 Sandia had modified two Mark 39 drop cases and was ready to drop the units from a B-47 at the Tonopah test range. However, on that same day, a meeting at Sandia between AEC and Air Force personnel recommended a B-52 instead, so the drop, which had by then been rescheduled for October 10, was canceled. On October 13 Headquarters Air Force directed Tactical Air Command (TAC) to deliver two C-130 aircraft to Kirtland not later than October 17 and 25. Air crews would be provided by AFSWP. Headquarters also directed Air Defense Command (ADC) to provide RB-57D aircraft for the air sampling mission and arrange that they arrive at Kirtland not later than October 17. The samplers would be maintained and operated by the Air Weather Service. AFSWC was directed to assist in the aircraft modifications and was designated as the Air Force point of contact and control for this program. Within the Laboratories, arrangements were quickly made for LASL to be responsible for one diagnostic aircraft and Livermore the other. EG&G were to arrange for fireball cameras and timing gear as soon as possible and the Laboratories took responsibility for time-interval and other instrumentation. AFSWC took on the job of preparing appropriate DME gear. The equipment initially installed in the 130s was whatever the Laboratories could find in-house, and some of it was primitive.

By mid-October Livermore had decided that they could be ready to drop a [redacted] as early as November 15, but equipment had to be installed in the 130s. Although it had been estimated that the installation would take several months, by October 28 it was intended to have the 130s completed by October 31, allowing an initial airborne diagnostic capability to be ready by early November 1961.

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The rest of the DOD also began to prepare for atmospheric testing in September. In addition to the September 19 letter transmitted through Seaborg to the NSC (mentioning the possible need for atmospheric testing), as noted before, DASA, on September 20, apparently in reaction to a September 12 memo from Harold Brown, authorized funds to address different types of measurements that might be made with high-altitude tests, including ionization caused by high-altitude detonation of nuclear weapons.

In spite of the feelings that politics would preclude its use, the Eniwetok Proving Ground was not forgotten. At a meeting of AEC, Laboratory, and H&N representatives on September 20 LASL described a quick two-month airdrop operation including ground-based measurements of yield and time interval, and Livermore described its tests, which were to be done from barges. H&N pointed out that (a) the

EPG base camp buildings would need electrical work, but were otherwise in good condition, (b) there were five cargo barges at Eniwetok which could be used as shot barges, and (c) they could obtain sufficient manpower quickly, but the necessary supplies of equipment and material would be a serious problem. Sandia suggested the possible use of balloons for the LASL and Livermore shots.

Bikini, however, would be a more serious problem because essentially all equipment and material had been removed to Kwajalein to support the Nike-Zeus program. The general conclusion seemed to be that a quick operation of a few shots could be done in two or three months, but that a better operation would require six to nine months preparation. This conclusion was confirmed at the Planning Board meeting on September 27, 1961. Subsequently, Sandia prepared a detailed proposal for balloon shots at Eniwetok using aerodynamic balloons (they had not finished the testing of these during the moratorium). The time required to prepare for balloon shots on both atolls was estimated to be six to eight months.

More Political Considerations

James Carr, Acting Secretary of the Interior, in a letter to Chairman Seaborg on November 3, strongly recommended against any further testing of atomic devices in the Trust Territory.

Kennedy kept trying to avoid atmospheric testing. On September 3 he and British Prime Minister Macmillan jointly proposed to Khrushchev a ban on atmospheric testing, to include monitoring by national means. On September 9 Khrushchev refused the proposal, calling it a "dishonest deal" since the U.S. had been preparing for underground testing and knew how to do it. On September 19 Seaborg recommended to the President that the EPG be brought to a three months readiness posture and that a seaborne operation be considered. On September 25 the President, in an address to the U.N. General Assembly, said, among other things, "a nuclear disaster, spread by wind and water and fear, could well engulf the great and the small, the rich and the poor, the committed and the uncommitted alike. Mankind must put an end to war or war will put an end to mankind. Let us call a truce to terror." He called for disarmament, stating that the logical place to begin was a test ban treaty.

By the end of September 1961 awareness of the need for atmospheric testing was growing. September had been a period of recovery from shock and early moves toward preparation for underground testing, but October saw accelerated preparations for atmospheric testing. Schlesinger has noted:*

> The urgencies of security, however, remained at war with the dreams of disarmament. Kennedy had felt that the Soviet atmospheric tests left him no choice but to authorise underground testing of our own. Now, as one explosion in the skies above Siberia followed another through the autumn, it became increasingly difficult to hold the line at underground tests. The Joint Chiefs of Staff, in particular, wanted to resume American tests in the atmosphere as speedily as possible. Early in October, they forwarded a paper calling for atmospheric testing in November. The JCS paper was below their usual level in logic and literacy. When we met to consider it at the State Department, Secretary McNamara, who had obviously not examined it with care before the meeting, quickly perceived its imperfections and abandoned it as a basis for argument. One defense official made an impassioned case for the resumption of atmospheric testing in order to prevent the world from believing that the Communists were gaining so commanding a lead that there was no point in resisting them further. But McGeorge Bundy replied that he was against tests for the sake of psychological warfare and insisted on the principle that we never test

*A. Schlesinger, *A Thousand Days*, page 486.

> in the atmosphere unless required by military necessity to do so. Then McNamara made it clear that a serious case for resumption existed in terms of military security, and the meeting ended with the recommendation that the United States take an early occasion to reserve its freedom to test above ground.

Gerry Johnson was quick to support Everready, and in the process triggered off a move that was to have major consequences in the operation. On October 2, after having talked to Kenner Hertford of ALOO and General Donnelly of Field Command, DASA, Johnson told Seaborg that he liked the Everready concept. The subject was discussed at the MLC meeting the next day, with both Betts and General Booth of DASA present. In discussing possible sites, Booth mentioned that DASA was looking into the possible use of Christmas Island, and H&N was looking at Eniwetok revival. Johnson pressed for identification of an alternative to the EPG because of his strong feeling that U.N. political pressures would make its use untenable. On October 10 Commander Holkum of DDR&E briefed Johnson about Christmas Island. Johnson apparently discussed the possibility with Betts in the next day or two and with Hertford, since Hertford later commented to Betts that if the Everready plan were accepted, it could also be adapted for operations from Christmas Island. On October 13 Booth sent Johnson a lengthy report on operational logistics on Christmas Island which had been prepared in 1959 by the Pacific Missile Range. Along with the report, Booth wrote:

> It appears that the adaptation of Christmas Island as a U.S. nuclear test site is both operationally and logically feasible. Christmas Island affords sufficient advantages to make it attractive as a base for sampling operations, balloon shots, and offshore detonations with onshore instrumentation.

He also stated his view that the selection of Christmas Island as a nuclear test site was second to Eniwetok, with which he was more familiar, and suggested that a survey of Christmas Island be conducted immediately. Later that same day DMA and DASA staffs decided to survey Christmas Island and suggested that the survey party include representatives from the DOD, the AEC, and the United Kingdom. The relative merits of the different facilities would be made after completion of the survey. Should use of the Christmas Island facilities be desirable, the DOD, AEC, and Department of State would jointly decide (a) how to approach the United Kingdom, (b) the desired U.K. participation in the tests, and (c) what information from the tests would be made available to the U.K.

The subject of test location arose again at the October 17, 1961, meeting of the Commission during Bradbury's discussion of the need for prompt resumption of atmospheric testing. He commented that Christmas Island might be another possible test site and added: "Since it is a British mandate, it would require the cooperation of the United Kingdom." [redacted]

On October 18 DASA sent to Gerry Johnson a list of suggested personnel to conduct the upcoming survey visit to Christmas Island. The list consisted of eleven military officers from the three services, including one doctor. The subject came up again in the Commission meeting on October 19, which was attended by the members of the MLC. In discussing the possible operations, Johnson explained to the Commission that Christmas Island had better weather conditions than Eniwetok and a larger, fairly level, land mass. He noted that Christmas Island was apparently disputed territory between the U.S. and U.K., but also commented that the U.S. had a big investment at Eniwetok.

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The DMA apparently did not agree with DASA's suggested personnel to visit Christmas Island, and by October 20 Anderson of DMA was organizing a reconnaissance party which included representatives of LASL, LRL, Sandia, and the ALOO OFO. The field system promptly began to scurry for information on Christmas Island in order to prepare the proper questions for the visiting team.

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The investigation became more urgent on October 29 when Dean Rusk, Secretary of State, informed Seaborg of his view that we should avoid using a site in the Trust Territory for any atmospheric tests and, hence, recommended strongly against the use of Eniwetok and Bikini. On November 1 Reeves told Betts the results of the test organizations' outlook on the use of Christmas, namely,

> On the basis of a permanent long-range test facility, it would appear that Christmas Island, from the standpoint of weather conditions, fallout problems, and international objections to testing activities, would have distinct advantages over Eniwetok. In the long run, any immediate savings that might accrue by use of existing support and scientific facilities on Eniwetok would be far outweighed by operational advantages of Christmas Island. It also appears that should Christmas Island prove unacceptable for high-altitude testing, a separate facility for this activity could be established at Johnston Island, and the increased cost and disadvantage of operating two sites would still be more than offset by the disadvantages of the combined facilities of Eniwetok/Bikini. This recommendation is based on one premise: that we are given complete operational control of Christmas Island--we doubt that joint operational control of Christmas Island would be acceptable.

Betts concurred in this recommendation in a message to Seaborg, and at the same time requested that a decision be made to conduct Project Everready from Johnston Island rather than Eniwetok.

Technical Developments

Early in October DASA called a mid-October meeting to accomplish advanced planning on high-altitude nuclear weapons effects testing, with the intent of formulating an overall test plan for blast phenomena and missile response and updating the Willow high-altitude balloon experiment plan. AFSWC, as a result of a meeting on September 28-29 with various Air Force agencies, presented the Air Force test requirements and objectives to Systems Command and the Air Staff on October 4 and 5, and to the USAF Scientific Advisory Board on October 6. On October 6 DASA began a study of flight safety and area impact safety problems for the Polaris and Atlas systems tests. On October 18 AFSWC and Sandia were jointly studying nuclear safety in the possible Atlas operation; Sandia determined that they could obviate the surface-burst problem by installing a shorting plug. On October 27 SAC briefed the Air Staff and the

Pentagon on the proposed Atlas system test. On October 30 DASA sent identical letters to the Chief of Naval Operations and the Chiefs of Staff of the Army and the Air Force requesting that by November 15 each Service forward to DASA their proposals for performing full-scale effects tests on the three high-altitude tests already designated (Starfish, Bluegill, and Kingfish), considering both a lead time of 18 to 24 months and an alternate of 9 to 12 months. DASA would use these proposals in making recommendations to the JCS.

By October 26 a preliminary operational plan for Everready was available. The operation would be divided into three phases. [redacted] would be scheduled sometime during the period from about November 15 to early February 1962. These detonations would take place in an area near either Johnston Island or Hilo. For the second phase tests, the weapons diagnostics would be a little more elaborate, as desired by LRL. The measurements would include fireball rate of growth, time interval by both electromagnetic and optical means, radiochemical yield, high-explosive transit time, and bhangmeter yield. Phase 3 was not particularly well defined, but was the imagined longer-range operation, which, in a sense, turned out to be Operation Dominic. Phase 1 would be performed within 7 to 10 days after authorization, whereas Phase 2 would not be ready until approximately December. The control organization would be an "air task group command" which, presumably, would be headed by McCorkle, with a Deputy Commander for AEC matters, presumably Jim Reeves. The air operation would include two B-52 airdrop aircraft, two RC-121 control aircraft, two C-130s and two C-54s for photo and instrumentation coverage, and appropriate B-57-B, -C, and -D aircraft for air sampling. Weather reconnaissance WB-50s would be needed and C-124s were needed for materiel transport.

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During October, in parallel with the concept of testing off Johnston or Hawaii over the open sea, provisions were also being made for testing at Eniwetok using the same capability, but having, in addition, ground-based diagnostic equipment.

As a result of the October 9 and 10 letters from Gilpatric and Seaborg, both of which urged returning to atmospheric testing, the President seemed to have given some half-hearted approval to proceed with plans and partial preparation. On October 12 McNamara authorized the JCS to proceed with preparation of plans on an urgent basis. That word, of course, was immediately transmitted through DASA to the Everready organization. By October 15 LRL was building multi-aperture optical systems needed to get time interval data. Sandia was preparing for trial drops of a 39 case containing a dummy [redacted] the first test to be from a B-47 on October 17 and the second test from a B-52 the following week. At the same time Livermore was preparing ground-based optical equipment for fireball measurements, and the Air Force was modifying C-130 and B-52 aircraft.

Initially there was trouble in obtaining sampler aircraft because AFTAC was using them to monitor the Russian tests, and the Laboratories could not agree on their needs. LASL requested at least one sample and said that more would be nice, but eventually their requests required three or four aircraft. Livermore, however, started with a request for five or six samplers and ended with as many as seven. At the same time, the possible desire to test [redacted] led Livermore to request a higher-altitude-capable sampler aircraft because the stablized cloud altitude from the high-yield test might exceed the ceiling of the B-57-B and -C aircraft. AFSWC's sampler aircraft problem was also difficult because several planes that might be available from USAF Systems Command had not been configured for sampling and McCorkle also had

Withheld Under 5 U.S.C 552 (b)(3) DOE, Exemption 3 to maintain a sampler capability for Nevada tests. In addition, trained pilots for sampling missions were at a premium. While other parts of the system had been degrading slowly during the moratorium, LASL had stored and maintained all of the sampler tanks obtained some years before and was able to use these on Everready and, for that matter, on Dominic. The aircraft situation improved when Air Force Headquarters directed ADC to transfer four B-57-D aircraft to Kirtland for modification there and directed the Military Air Transport Service (MATS) to transfer seven B-57-C and two B-57-E aircraft to Kirtland, with modification to be accomplished at Warner-Robins AFB prior to transfer. Eventually the LASL request was for four B-57-Bs and two B-57-Ds for high-altitude sampling, and LRL needed two B-57-Bs and five B-57-Ds for shots over a megaton, but if the D models were not available they would accept a force of six B-57-Bs. By October 27 thirteen of the B-57s were being modified for sampling.

This period also saw the beginning of an experiment that was to continue through the Christmas Island operation. On October 16 the medical division of DASA sought HQ DASA approval to do retinal burn-threshold studies on Everready. It was felt that preparations could be completed in three weeks if C-118s or C-130s could be used. The experiment would consist of measurements of light flux using appropriate equipment and, in addition, studies of retinal burns in monkeys and rabbits.

On October 23 Air Force HQ agreed to the experiment and directed Systems Command to furnish eight aircraft to be operated out of Hickam Air Force Base for about eight days in the period from November 13 to November 21, 1961. The designated aircraft, in order of preference, were C-118s, C-113s, C-119s or C-54s.

On October 20 Air Force Headquarters asked the Navy for permission to use Barbers Point for operation of certain aircraft, especially sampler aircraft, because of problems radioactivity would cause at Hickam.

By October 21 Livermore was planning to make an early alpha measurement (along with the other diagnostics) if the airdrops were near Johnston Island. (Note that the drops had to be within aircraft range of their operating island base.)

On October 26 Gerry Johnson and Seaborg discussed the need for qualified, experienced people to serve as safety advisors to the operational commanders during the series, and suggested Graves and Batzel as possibilities, amongst others. On October 26 Livermore named Bob Goeckermann as the LRL Test Group Director for Everready, but by the end of October Bradbury was still uncertain about the Everready organization and was not willing to appoint a LASL member of the Everready staff. On October 27 Headquarters Systems Command notified CNO, CSAF, DASA, the major Air Force Commands, etc., that General McCorkle of AFSWC had been appointed Air Task Group Commander for Project Everready.

The system began to come apart in the last week of October. On October 24 a test unit identical to the one intended for actual airdrop testing was dropped at Tonopah from a B-52 at 45,000 feet. The unit detonated at a position only 3,000 feet below the aircraft instead of 3,000 feet above the ground. The B-52 suffered no damage. On October 26 Henderson of Sandia informed Betts that the suggested nuclear drop date obviously could not be met, and on October 27 AFSWC notified Air Force Systems Command to the same effect.

Growth of the Task Force

DASA could see the handwriting on the wall: on October 3 they established a "test coordinating group" within Headquarters DASA to:

a. Prepare contingency plans for the conduct and support of possible high-altitude open sea and large-scale overseas tests involving nuclear weapons.

b. Develop recommendations for the command and control relationships of nuclear test organizations including consideration of the early activities of a "skeleton" joint task force.

c. Define areas of DASA staff responsibility to ensure complete coordination of staff efforts in this regard.

d. Effect coordination of DASA test planning activities with the military services, AEC, and other governmental agencies, as required.

The group chairman was Brigadier General Douglas C. Polhamus, U.S. Air Force. The initial group had 11 members, including Colonel Thomas L. Mann, U.S. Army Infantry, who had been the Commander of Joint Task Force 7 when it was dissolved in 1958.

In conjunction with this activity, a test coordinating division was established under the Deputy Chief for Operations, Colonel Mann. Among other things, this division would serve as the central staff agency for coordination with other agencies, would prepare plans and programs for nuclear weapons tests as directed by the test coordinating group, and would serve as secretariat to the test coordinating group. On October 9 Polhamus directed that the group prepare a recommendation for activation of a skeleton task force organization.

The October 10 letter from Seaborg to the President on the need for atmospheric testing mentioned that a military task force would be required for logistics support. On October 12 the Chief of DASA, referencing McNamara's memorandum that had transmitted the Presidential acquiescence to some preparation, advised the director of the Joint Staff:

> For the overseas tests, it is considered necessary that a Joint Task Force be established to develop detailed operational logistic plans and conduct the operation. Chief, DASA, would supervise the effects portion through a technical group in the JTF. It is recommended that the JCS direct the establishment of the JTF and designate one of the Services to provide the Commander thereof. I recommend the JTF be initially established, manned, and operated under control of Chief, DASA, with provision for separate operation under the JCS at the appropriate time to conduct the overseas tests.

On October 24, 1961, implementing an instruction from the Joint Chiefs of Staff received earlier on that day, Chief, DASA, announced the activation of Joint Task Force 8.

While awaiting the JTF-8 personnel, Polhamus continued to plan. At his meeting on October 25 he discussed plans to have 69 people on board in 30 days, 183 in 90 days, and 228 in 120 days, and pointed out that $1,000,000 had been obtained as initial funding, with the first year cost estimated to be about $40,000,000. He noted that General Starbird was expected to be the Commander, with General Lampert as the alternate. JTF 8 planning would continue under Colonel Mann until the Task Force was functioning. Office space had been requested either in the Pentagon or at least nearby. His second weekly planning meeting on October 31, designated a JTF-8 meeting by Polhamus, included representatives from the Army, Navy, Air Force, and AEC. The meeting agenda included reviews of all known plans for atmospheric testing, including Everready, the ASROC, Polaris, and Atlas systems tests, and the proposed high-altitude tests. A Navy representative stated that "The ASROC test is ready to go. Some ships are presently at sea. The longer this test is held up, the greater the dangers

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of information leaking to the public." The Navy also presented a Polaris system test plan which included use of the Atlantic Missile Range and impact southwest of Ascension Island.

On October 31, 1961, DASA informed the Assistant Secretary of Defense for Public Affairs that the JCS had ordered DASA not to announce the name, role, or existence of the task force organization until specifically authorized to do so. However, DASA suggested that appropriate officials of the DOD and AEC request approval from the President to announce the formation of the organization as soon as possible.

The Pressure to Resume

The pressure on the President to resume atmospheric testing was growing. On October 7 Seaborg urged Rusk and McNamara to be cautious at the upcoming United Nations General Assembly. He suggested that the President not agree to any resolution that would curtail our resolve to resume atmospheric testing and that we not enter into another uncontrolled moratorium under any circumstances. Seaborg also pointed out to the President the difficulties in underground testing and said that atmospheric testing would be a necessary supplement to the current underground program if the program needed to be accelerated. He also stressed that this status report was not intended to be a recommendation for atmospheric testing at this time. On October 9 the President received a Gilpatric letter which outlined a possible atmospheric series, along with appropriate justification, and recommended approval to prepare for atmospheric and high-altitude tests. The Gilpatric letter pointed out that:

> It is fallacious and dangerous to our national security to assume that we have reached a favorable plateau in nuclear weapons development, and that extensive efforts in nuclear testing are no longer required. On the contrary, from past experience, we know that nuclear testing has enabled our scientists to make extraordinary progress, not only in weapon technology, but in the discovery of previously unknown and unsuspected phenomena. We believe that similar gains can be made in the future.

As already mentioned, the President apparently gave a little at this point; at least McNamara authorized DOD planning and some preparation.

But the President still tried to avoid atmospheric test resumption. On October 13 Arthur Dean challenged the U.S.S.R. to sign an immediate test ban treaty, and warned that if the Soviets continued explosions, the U.S. might test in the atmosphere.

At the AEC meeting of October 17, 1961, the Chairman noted the extent of the U.S.S.R. series to date (some 20 shots, [redacted] and said there is little doubt that the U.S. must establish a testing program to meet its requirements, and not act only in response to Soviet-inspired pressures. The Commissioners agreed with this viewpoint and noted that Ambassador Dean had expressed a similar view. Bradbury urged that the AEC prepare the Eniwetok site and resume atmospheric testing as soon as the DOD could support an airdrop test program. He added, however, that if Eniwetok was not available, there were many other places in the Pacific that could be considered. At the same meeting:

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> [redacted] Mr. Foster said that he would have preferred doubling the effort in this area of development, but such a stepped-up pace is not possible in view of the level of effort

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> required in the current testing program. The Chairman observed that the Commission must keep currently informed on new developments in fusion weapons research because of recent widespread publicity regarding the development of the neutron bomb. . . . Mr. Foster said it is still difficult to convince the personnel at Livermore that the U.S. is once again engaged in full-scale testing. He cited the contrast between General Betts' directives, which stress the urgency of the program, and the President's public announcements, which indicate a strong preference for continued negotiations and moratorium. He said it would be most helpful if the Commission would clearly inform the University of California of the urgent nature of the situation.

The growing awareness of the magnitude of the Russian program was a further pressure on the President, and to add insult to injury, on October 17 the Russians announced their intent to fire a 50-megaton atmospheric detonation. For the next few days there was a continuous exchange between the President and other members of the government on the effects of such a detonation, possible use of such a weapon, etc. At an October 19 meeting of the Commission's General Advisory Committee the evidence became clearer. Scoville summarized the U.S.S.R. test program, pointing out

> . . . that [redacted] Soviet devices had been shot, and only this same morning, a report had been received of numbe[redacted] . . . shots occurred at Novaya Zemlya, Semipalatinsk, and Kapustin Yar.[redacted]

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5 U.S.C. 552(b)(3)
DOE Ex 1&3

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DOE, EXEMPTION 3

A little later in the meeting Seaborg commented that:

> Both Laboratory Directors feel that since progress would be extremely slow in preparing for a sufficient number of underground shots, the U.S. must test in the atmosphere. Since underground testing does not provide good diagnostic data quickly, since the Russians have not worried about fallout, and since the international repercussions over the Russians' tests have been considerably less than anticipated, the AEC has felt it should resume atmospheric testing and has recommended to the President that it be authorized to make preparations for atmospheric testing anywhere.

The Commission went on to review the entire proposed program presented by the DOD in its letter to the President on October 9. [redacted] On October 24 the Soviet Minister of Defense stated that, "The problem of destroying rockets in flight has been successfully solved." On October 27 another small Russian high-altitude test was noted.

There were other actions that day. The U.N. General Assembly asked the U.S.S.R. "to refrain from carrying out their intention to explode in the atmosphere a 50 megaton bomb." The AEC Chairman sent the President those recommendations from the AEC General Advisory Committee that the Chairman, Mr. Pitzer, had requested be

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communicated to the President without delay.

> **The Committee advised the Commission that they are of the firm opinion that militarily useful technical information can best be obtained by atmospheric testing. Secondly, it would be technically feasible to conduct a useful atmospheric test before the announced termination of the current Soviet series on October 31, if a decision were made to resume such testing within the next few days. The Committee believes that possible political advantages of such a test should be evaluated promptly. Third, the Committee is convinced that the AEC could, within a few days of a Presidential directive, come up with a single weapon having a yield of about 50 or maybe up to 100 megatons.**

In his forwarding letter to the President the AEC Chairman said that the General Advisory Committee was overly optimistic about the short-time availability of a 50-megaton device.

Governor Rockefeller of New York also urged resumption of atmospheric testing: "To assure the sufficiency of our own weapons in the face of the recent tests, we are now clearly compelled to conduct our own nuclear tests." The Governor, who was a potential candidate for the Republican Presidential nomination in 1964, further pointed out that if the United States fell behind the Soviet Union in nuclear weapons it would pave the way for Communist conquest of the democratic world. He commented further, "It is one thing for America to be conscientiously concerned with the views of neutralist nations. It would be quite a different and preposterous thing for America to start behaving like one." Also, on October 27 Communist China broadcast an urgent warning against radioactive fallout in its northern provinces and offered health advice on the subject.

On the 28th Khrushchev announced his intention to go ahead with the 50-megaton shot, and complained, "Bourgeois propaganda, as of late, raised a clamor around the fact that the Soviet Union has been forced to resume nuclear weapons tests," adding that the Soviet motivation in proceeding with the test was not properly understood.

On October 30 Seaborg reviewed for the President the present status of test plans, including the proposed military tests. He specifically mentioned the problem of the EPG and [redacted] gave his own suggestions for restraints on the program, endorsed the position of the Committee of Principals in their October 11 memorandum to the President, and passed on the Commission's opinion that the national security now required the U.S. to test in the atmosphere at the earliest appropriate time, minimizing the U.S. contribution to worldwide fallout. Finally, he stated, "In conclusion, I respectfully reaffirm our earlier recommendation that the U.S. forthwith proceed to full-scale preparation for atmospheric tests, and that those preparations be publicly acknowledged as recommended by the Principals."

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On that same day, October 30, the U.S.S.R. exploded its 50-megaton bomb. On the following day Prime Minister Macmillan declared that the United Kingdom would support a U.S. decision to test above ground, saying, "We cannot risk putting the West in a position of permanent military inferiority."

There was widespread reaction to the Russian test. The Vatican Radio termed the blast an "insane decision, morally, politically, socially, economically, and humanely deprecable" that "shows the true face of Communism . . . a face with the light of love and reflecting the tension of hatred." A West German spokesman charged that the Soviet Union "was ruthlessly risking the health of all mankind." The explosion was taken as a new proof of Moscow's "brutal determination" to display its military power. A member of the Storting* in Oslo, displaying anger shared by all parties

*Norwegian Parliament.

there, said, "The explosion showed a cynicism unparalleled in history." The New York Times issue for October 31 showed a map of the damage to New York City from a 50-megaton bomb explosion in the air above Wall Street: there would be fatal burns to exposed persons as far as 35 miles from the detonation. At the U.N. on the 31st, Stevenson said, "If this is what Mr. Tsarapkin calls 'Soviet realism,' God help us all to escape from Russian realism." By this "arrogant act" Stevenson charged the Soviet Union has "added injury to insult" and has "started a new race for deadly weapons . . . and has contemptuously spurned the appeal of the United Nations and of all peace-loving people." Mr. Godber, British Minister of State for Foreign Affairs, told a news conference: "We are still ready, in spite of this latest shocking act by the Soviet Union, to go back to Geneva and try to make a treaty. But if that is not done, then we must reserve our own right to act."

On October 30 Senator Henry M. Jackson, Chairman of the Joint Subcommittee on Atomic Weapons, said that the United States would have to resume nuclear tests in the atmosphere, pointing out that "There could be no question that the Soviets are improving the sophistication of their warheads to the point that the long lead we have may be in jeopardy."

On October 31, 1961, at an MLC meeting, Colonel Anderson of DMA commented:

> that the top-level AEC people were by no means proceeding so enthusiastically with test planning as were the DOD top-level people. In DMA, they do not have the direct guidance which we have in the DOD. The AEC has not faced up to the need for planning for atmospheric testing and operating with JTF 8. The only "joining" between AEC and DOD at present is at the AFSWC-ALOO level. . . [redacted] was dropped and went off at 14 seconds instead of 40 seconds and DMA had their enthusiasm dampened for any hurried preparation for airdrop.

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(The author finds this a strange remark since at that point the AEC was ready to drop two stockpile devices within a few days and could within the next month, in principle, airdrop a number of other devices, whereas the DOD could do only systems tests.) At the same MLC meeting, Gerry Johnson summarized recent discussions involving McNamara and Seaborg which had led to the DOD position that selection of a site for atmospheric testing should be a single-agency decision. On October 31 the AEC agreed that it should have primary responsibility for site selection, with the exception of possible early drop tests, and this decision was passed to the Secretary of Defense.

Arthur Schlesinger reported:*

> On the morning of October 30, a call from the White House awakened me to report the largest detonation so far, probably that of Khrushchev's threatened 50-megaton bomb. . . . This final atrocity made it impossible to put off our own preparations for atmospheric testing any longer. Kennedy now directed Ted Sorenson to draft a statement saying that while we should test in the atmosphere only if required to do so by overriding arguments of National Security, contingency preparation should begin at once. Three days after the great Soviet explosion, the paper was laid before the National Security Council. . . . The meeting had begun with the preliminary analysis of the Soviets tests. The new Russian series, according to the CIA report, followed logically from its 1958 series, this suggesting that in spite of the "big hole" thesis, there had been no cheating in the interim. Then McNamara, after an impressive and dispassionate review of our weapons situation, asked that development and effects tests in the atmosphere be authorised at the earliest possible moment. The President inquired about the timing of the projected series and said that if we had to have the tests, they should be run off rapidly; "we want to do as little as possible to prolong the agony."

*A. Schlesinger, *A Thousand Days*, page 487.

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On this note, the meeting adjourned.

At the end of the day the President announced publicly that preparations were under way for atmospheric tests "in case it becomes necessary to conduct them." They would not be undertaken, Kennedy emphasized, "for so-called psychological or political reasons." But if the "orderly and essential scientific development of new weapons has reached a point where effective progress is not possible without tests," then they would be undertaken "within limits that restrict the fallout from tests to an absolute minimum."

Thus came the orders from senior authority to prepare for testing. It is interesting to speculate about why testing was not to start immediately. The Task Force was not yet operational, but the ASROC test was ready. The Polaris test was ready, and while there had been trouble with [redacted] presumably was droppable. Furthermore, within a few weeks either Livermore or LASL could, in principle, have had other devices ready to drop. Such an operation would have been a high-risk affair since very few of the appropriate safety systems had been set up (such as weather stations and rad-safe organizations). We can only suspect that the Polaris, ASROC, [redacted] simply did not meet the ground rules of immediate necessity. Furthermore, the President had satisfied the AEC and DOD by allowing them to prepare, which was really simply the expenditure of a fair amount of effort and money, but he had maintained the option of continued negotiation of a test ban. It is clear that the President's objective was not for the U.S. to test, but to prevent any further Russian testing. The open declaration of our intention to prepare for atmospheric testing could, in a way, be regarded as pressure on the Russians to move toward a test ban treaty.

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Be that as it may, the testing system now moved rapidly toward achieving that readiness. While the program would not be defined officially until the [redacted] meeting, the elements were clear enough for AEC and DOD action.

The Preparatory Period

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November 1961, as related in previous sections, was a period of program and concept definition. While consideration of Eniwetok continued for a while and Christmas Island began to be a gleam in the tester's eye, it quickly became clear that an open sea operation of some sort was the only concept that would be politically acceptable at the moment. But Everready lingered on. On November 1 the Air Force changed the nickname Everready to Bluestraw and defined that project to be Air Force support of nuclear testing. (The name Bluestraw for that Air Force support continued long after the end of Operation Dominic.) The Laboratories quickly realized that the concept of airdrops from a B-52, using diagnostic airplanes such as the C-130s, had to be preserved, at least for a while. Thus, even though the status of Everready was quite uncertain, the AEC Laboratories, with the help of EG&G, continued to increase the diagnostic capability of the C-130s. Livermore, in conjunction with Sandia, was preparing radar tracking and ground-based optical systems for both optical time-interval measurements and fireball photography. On November 3 CINCPAC (Commander-in-Chief, Pacific) informed Navy units of the Bluestraw operational concept, specified their responsibilities to clear and monitor the designated drop zone, which was designated as a 200-mile square centered 350 nautical miles southeast of Hilo, and stated that the series would begin November 15. Naval aircraft support was also specified.

However, on November 4 Betts informed the Laboratories that, among other things, the new readiness date was about March 1. On November 7 Bob Miller of ALOO notified

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AFSWC that the Everready operation was cancelled, and AFSWC began to turn off that effort. On November 8 MATS notified its subordinate units that the nuclear tests would not be implemented in the immediate future, but certain aircraft, such as the photographic and air weather aircraft, would be retained in modified configuration for possible reinstatement of the project. On November 8 McCorkle commented to Schriever (Commander, USAF Systems Command) on the disruption that had occurred during the last 30 days, suggesting, therefore, the need for a permanent organization at AFSWC to cope with the many facets of such an operation, and stated his intent to complete an organizational plan which would be submitted for approval later in the month. On November 14 TAC requested that AFTAC advise when the C-130 aircraft could be returned to TAC. On November 24 Systems Command replied to TAC that the two C-130-B aircraft on loan to AFSWC were required for a new program and that a firm return date was not available.

The AEC Laboratories, EG&G, H&N, ALOO, and AFSWC now began to define the operational concept in greater detail. Initial estimates of the safety hazards were quickly made within the Laboratories, especially by Orin Stopinski of LASL and Vay Shelton of LRL.

On November 2 Betts sent to the Laboratories a list of instructions, which included:

> We must plan for an intensive atmospheric program on a relative short time duration to be executed this spring (assuming that the decision to resume testing is made). More specifically, it appears that such a program will start on or about March 1 and will last for 2 to 3 months. There is no assurance that another atmospheric test program will be repeated after the initial series is executed; however, we have been instructed to plan for a similar atmospheric test series on an annual basis. . . . The location for the United States testing will be in the Pacific at a location presently undetermined. The AEC is charged with the determination of a suitable location-- you will be advised of our efforts in this regard by separate communications. . . . We are currently negotiating with the U.K. for the use of Christmas Island as a first choice for an island base. In the event that Christmas is not available, the use of the Eniwetok-Bikini Islands will be reconsidered. Meanwhile, studies will be made of other possible suitable island sites. Parallel planning will continue for early capability to conduct developmental tests by employment of an Air Task Force based in the Hawaiian Islands with detonations to occur southeast to southwest of Hawaii and utilizing Johnston Island, as appropriate, dictated by weather and other considerations.

He then called for a meeting on November 13 with all participants at Albuquerque. The following conclusions and recommendations from the November 11 meeting of the NTS Planning Board were presented to Betts and the testing principals at their November 13 meeting in Albuquerque:

a. Priority of Desired Real Estate Based Upon Maximum Capability

   (1) Eniwetok/Bikini
   (2) Christmas Island
   (3) Johnston Island or Hilo, Hawaii

b. Conditions Associated With the Utilization of Christmas Island

   (1) The earliest possible authority should be obtained for an on-site survey of Christmas Island; early authority should also be granted to accomplish support action to attain March 1, 1962, readiness.

   (2) Ideally, conditions for use of Christmas Island should provide for:

(a) Sole U.S. operational control.
(b) Permanent removal of native groups.
(c) Acceptance of the principal that under conditions (a) and (b) above and subsequent paragraphs, the test organization may, after several years, still obtain at best only 50 percent of the capability attainable at Eniwetok/Bikini.

(3) Sampler aircraft should be based at Christmas Island.

(4) Capability to measure early alpha must be developed; this includes two alpha stations to support balloon, air, and barge shots, thus providing one alpha station for each laboratory.

(5) Requirement for at least three camera stations for airdrop, balloon, or barge shots, these stations to be used jointly by both LASL and LRL.

(6) Additional camera stations to cover single-stage devices.

(7) Should Eniwetok/Bikini or Christmas be unavailable, necessitating Pacific shots to be fired at Johnston or off the Hawaii coast from Hilo, additional devices must be tested at NTS -- to obtain alpha -- and in the Pacific area -- to obtain yield.

(8) Certain events may be so difficult that barges or ships may be required as diagnostic platforms for detonations off Christmas Island.

(9) Balloon preparations must be initiated immediately if the March 1 readiness date is to be attained.

(10) Logistics, personnel, and other factors must be surveyed to determine the extent of the support problems.

c. Conditions Associated With Johnston Island or Hilo

(1) Provided Eniwetok/Bikini or Christmas Island is not available, it is possible to use either Johnston or Hilo; however, the operation would a mixed air and surface ship operation, and diagnostics programs will be restricted.

(2) Fireball equipment installed in aircraft should be retained, ready to support off-Hilo or Johnston operations.

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DOE, EXEMPTION 3

(3) The [redacted] test should be planned for a ground site. This would permit utilization of a vacuum system as part of diagnostics. As an alternative, it may be feasible to utilize a missile system staged from Johnston Island.

(4) Johnston Island is too small and too restricted, and an extended program would require additional locations.

d. Reemphasized Advantages Associated With Operations at Eniwetok/Bikini Atolls

(1) Maximum separation, permitting dual laboratory preparations for detonations.

(2) Weather conditions, subject to further analysis, which may be only slightly less acceptable than those at Christmas Island. (Additional weather studies relating to Christmas Island are being directed.)

(3) Land separation reduces the long-life contamination of ground areas which will be inherent in operations at Christmas Island.

The meeting of the testing principals in Albuquerque on the 13th, which included senior representatives from each of the AEC Laboratories, EG&G, Field Command DASA, AFSWC, ALOO, and others, noted the Planning Board's recommendations on the EPG, but concluded that that site was politically infeasible and recommended that planning should be directed to Christmas Island, with the alternatives of Johnston and Hilo. Most proposed device tests could be built for airdrop, but a few, which could not be airdrops, required a barge, balloon, or ground site. It was also noted that all events might require a sea vessel of some type as a control and observation vehicle, so a joint air-sea operation would be required. It was agreed that support requirements would be developed for three operations concepts, as follows: operate completely from Christmas Island, operate in part from Christmas Island, or operate from Oahu or Johnston over open water. Other conclusions included (1) requirements for three phototrailers for measuring fireball yield, (2) wing tanks and associated equipment to support airplanes assuming two missions per day on two successive days, (3) two trailers, one for electromagnetic and one for Teller-light time-interval measurements, and (4) two alpha measurement stations, each equipped with 40 oscilloscopes and designed to withstand 300 psi blast overpressure. The alpha stations would be located at two balloon-equipped test sites capable of shots as large as 100 kt. Other equipment needed at the balloon sites included either 15 balloon winches which would be destroyed in the tests or three reusable winch trailers capable of withstanding 100 psi blast overpressure. Still other facilities were (1) rocket or missile launch facilities for one or two shots at Johnston Island, (2) two radar tracking trailers, (3) two telemetry-type trailers to observe weapon functioning, (4) a shop trailer, (5) decontamination fresh water facilities for aircraft, (6) two Boxer-type ships for diagnostic platforms, (7) 40 to 50 aircraft of several types, (8) anchoring and barge facilities for fuel handling, and (9) five weather islands. The total personnel, including the Laboratories, the air support, and DASA and their support, but not including construction people, was estimated to be 2,410 people. It was assumed that some of these people would be aboard ships and the others would be in tents or other quarters ashore. It was concluded that early permission was needed to visit Christmas Island and to initiate support action there. The DOD listed Starfish, Kingfish, and Bluegill as their test requirements. It was noted that the fireball optical equipment already installed in the C-130s should be maintained, that there was an increased requirement for high-altitude sampler aircraft and crews, and that additional study should be made of sampling techniques utilizing rockets. It was also recognized that the possible effects of air blast and flash blindness might lead to the airdrops near the Hawaiian islands being farther away than previously anticipated, complicating the airborne sampling problem even more.

Changes to the detailed concepts of the November 13 meeting came rapidly. On

November 16 DMA staff personnel suggested to Betts that the Commission be asked to authorize an open sea operation immediately. However, Al Graves pointed out the additional hazard of tsunamis in Hawaii in case of an accidental surface burst. LASL and Sandia representatives met on the 16th to refine the plans for the atmospheric program. A possible high-altitude method of measuring neutron distribution from the [redacted] was proposed. This would involve lifting the device to 150 kilometers, turning it on its side, and detonating. Observations would then be made by detectors lifted to about 200 kilometers using small sounding rockets which might be fired from Johnston, Midway, Kauai, Christmas, Palmyra, or Jarvis islands. The device might have a thin lead shield on one side to check out the "lead balloon" evasion theory. In other discussions Sandia agreed with the LASL request to take responsibility for early alpha measurements using telemetry, as they had done on all LASL shots of Hardtack Phase II. Pending further investigation, LASL agreed to Sandia's preference of the TX-39 drop case for all airdrops, regardless of the size of the device. Sandia also agreed to monitor the various device functions on airdrops, including squib firing, X-unit firing, supercharging, etc.; to furnish the radio signals; and to start the timing signals for such airdrops. Sandia was already preparing to furnish a ground-based system for tracking the "drop plane and device," thus preventing the kind of data loss that happened on Cherokee. The gear could be put on ships if necessary. Sandia had already ordered 25 balloons in two sizes, one that could lift 1,800 pounds and the other perhaps 15,000 pounds.

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On November 17 Ogle informed Betts that LASL had changed some diagnostics requirements since the November 13 meeting. Fireball camera stations would be required on the surface and in the air independent of test location and would be operated by EG&G under LASL direction on LASL shots. Time interval would be measured similarly, from ground stations, by both LASL and LRL, and might also be attempted from the C-130s. On any single-stage device to be fired at Christmas using a balloon, fireball yield would be obtained from ground stations only. On LASL shots bhangmeters would be operated by EG&G and the data would be interpreted by LASL. Both a ground surface shot on Christmas and a high-altitude shot would be considered for the measurement of neutron distribution from the [redacted] and no choice had been made. On the deep space shot intended to develop diagnostic measurements for possible future space testing, x-ray intensity measurements in space would be made jointly by Sandia and LASL (and possibly LRL). Ground-based and airborne optical measurements of x-ray intensity, time interval, and atmospheric characteristics by observation of air fluorescence on all high-altitude shots would be made by LASL from stations on Johnston Island and from high-flying C-135 aircraft.

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Later discussions led to the conclusion that neither steel nor wooden shot towers could be erected in the time allowed. Consequently, Livermore would have to consider some other means of firing. Livermore alpha stations could be ready 13 weeks after go-ahead, which would be 10 days before the required readiness date if go-ahead were immediate. LASL was planning two shots on floating platforms and two or three missile tests, each of which would require about 25 companion rockets. The LASL and LRL alpha stations would be very similar. It was agreed that all shots on floating platforms would be fired by radio, except that LRL would request hard wire to barges, provided the moorings were not too far from shore. H&N was authorized by the AEC to proceed with engineering on the Livermore criteria, to begin negotiations immediately for the purchase or rental of construction equipment, and to arrange for barge tows. Estimates of the funds required were as follows: H&N construction support, $26,000,000; Sandia, $17,000,000; EG&G, $14,500,000; total, $57,500,000. Half would be committed by March 1, 1962, for an operation beginning on that date.

At the November 17 Commission meeting Betts suggested that:

> Unless final negotiations for Christmas Island can be accomplished quickly or support for Eniwetok/Bikini operations can be obtained from the highest governmental levels, I strongly but reluctantly recommend that decision be made to conduct the test by airdrop or barge shots in the open sea. I feel that a decision at this time will provide the guidance needed to place all technical and operational preparations on a systematic basis. With the first knowledge that the tests will be conducted at sea, all efforts can be applied in this direction and it is likely that improved techniques can be worked out that will overcome the inherent disadvantages of such an operation. Continued delay in selection of a test site will greatly increase the cost in terms of funds and manpower, as well as reduce the effectiveness of final operation, since effort must be directed to support several contingencies instead of supporting a specific plan of action. In summary, I recommend that unless there is a good possibility of obtaining Christmas Island or Eniwetok/Bikini atolls by December 1, the Commission make a decision to proceed with an open sea test operation, making use of Johnston Island and Hawaii support facilities as feasible. If it appears that agreement for use of Christmas Island might be obtained with extended negotiations, the negotiations should be continued in order to provide a more suitable place of operation for testing in the future.

On the 18th Luedecke briefed the JCAE on present atmospheric test planning, including the President's designation of the Seaborg-chaired NSC subcommittee as the organization to review and recommend U.S. atmospheric test plans. On November 20 both Sandia and EG&G submitted to Reeves their detailed estimates of equipment and costs needed for a test series based on Christmas Island. Jim Carothers of Livermore named Chuck Gilbert as his Deputy Test Director for Pacific Operations and made Jack Shearer responsible for the diagnostics and experiments on those events. On that same day McCorkle of AFSWC discussed with Systems Command Headquarters the AFSWC concept of an Air Task Group to support the upcoming atmospheric nuclear testing as part of the Joint Task Force. After recalling previous experience and noting that the 4950th had been discontinued on August 16, 1961, he proposed to establish a "nuclear test mission element" within AFSWC with an initial manning of 20 people. He noted that with augmentation this could become a provisional Air Task Group under a Joint Task Force. He estimated that 85 people would be required for the Air Task Group if it were based at an established air force base and suggested a much greater number would be required if it were located elsewhere.

In his letter to the President after the November 21 National Security Council subcommittee meeting Seaborg noted:

> The choice of test site will dictate how the tests can actually be conducted. Technically, the Eniwetok Proving Ground is the most desirable, extending as it does over a substantial area, with a lagoon suitable for barge shots. However, the contemplated tests could probably also be conducted, but not so well, at Christmas Island. Since Eniwetok has political difficulties and the availability of Christmas is at best uncertain, prudence dictates that we be prepared to test elsewhere if necessary. Fortunately, many of the proposed tests could be conducted without a highly developed island site, although they would benefit from such a site. Some could be done by airdrops probably straight from Hawaii with limited instrumentation on some small island, such as Johnston, not suitable for more extensive development; with some degradation of diagnostic information, others could be carried out by airdrop over the open ocean using such instrumentation as could be carried in accompanying aircraft or on ships. However, some of the most complex instruments are of questionable feasibility except over an extended land base such as Christmas Island; in the absence of such a base, serious consideration should be given to conducting some of these above ground in Nevada.

He also noted that as directed by the President, the new planning target date was April 1, 1962.

On November 30 Batzel and Goeckermann of Livermore sent Betts a summary of their intended diagnostic program. It was essentially a mirror image of the LASL program with the word LRL replacing LASL. Balloon and barge shots were assumed along with airdrops. However, they noted in particular that some measurements on large weapons fired on barges would be complicated because of line-of-sight difficulties, and they were, therefore, relying on airborne disgnostics. They noted that recent dry runs using the C-130 aircraft had convinced them that several improvements were needed. The X-unit signal from the drop case was not large enough; no method existed for dry running the RF pickup and optical gear while the aircraft was in flight; and excessive vibration had caused a number of instrument failures during full power checks on the ground. They therefore requested that the C-130 assigned to LRL be made continuously available from that moment throughout the test series for development and testing use. On the space shots, LRL proposed to make x-ray intensity, primary alpha, time interval, and neutron time-of-flight measurements themselves, with Sandia being responsible for rocket firing and telemetry. The diagnostic packages would be flown on sounding rockets launched from Kauai and Johnston. If an LRL device were used in any of the high-altitude shots, they might attempt radiochemical sampling.

By mid-November much of the planning responsibility had been assumed by the Joint Task Force. General Booth, Chief, DASA, had moved quickly after the October 24 authorization to establish Joint Task Force 8. To be Task Force Commander, Gerry Johnson had specifically suggested Starbird, whose previous experience and long tour as head of DMA made him an obvious candidate. On November 15 the charter of JTF-8 was still being held up pending arrival of General Starbird, presumably so that he could help in its formulation.

At the November 16 Commission meeting Luedecke introduced for Commission consideration the appointment of Starbird as Commander of JTF-8 and his designation as the senior AEC representative at the overseas testing site. The minutes of the meeting note that Mr. Graham discussed the point:

> First, he said, it is important to establish a firm delegation of responsibility to the Commander in matters affecting the health and safety of the public which may arise in the course of the testing operation. He said the second important aspect is keeping the AEC fully informed so that the Commission, in turn, may notify the President and the JCAE of the developments which may arise in the course of the testing operation. General Betts stressed that as AEC senior representative, General Starbird will be directly responsible to the Commission and he will be required to abide by AEC standards for assuring the health and safety of the public. General Starbird will also be required to keep the Commission fully and promptly informed.

The Commission concurred in the appointment of Major General Alfred D. Starbird, U.S.A., as Commander, Joint Task Force 8, and noted that the Chairman of the MLC (Gerry Johnson) would be advised of this action by letter, which would also indicate the Commission's intention to appoint General Starbird as the senior AEC representative at the overseas test site for the operational phase of the test operations. It was decided that no public announcement of the appointment would be made and that the JCAE would be advised by appropriate letter later.

The first Task Force General Order, on November 21, 1961, shows that General Starbird assumed command on that day in compliance with the November 2 direction of the JCS. When called to the new assignment, he had been on the west coast serving as Chief of a Corps of Engineers field office, and he had to take some time to settle affairs there and move his family. He apparently had flown east early in November to discuss the appointment with the JCS and others, stayed there a few days, and then returned to move his family. In mid-November, after checking with Bradbury and others, Starbird asked Ogle if he would be willing to act as the Scientific Deputy Commander of JTF-8. After checking with Graves and Bradbury, Ogle quickly agreed.

On November 20 Starbird and Ogle met in Denver for a few hours as Starbird was driving back across the country with his family. At that meeting they agreed on a manner of operating and their appropriate separation of duties. It was very simple: Starbird would concentrate on the Washington problems, the military problems, site agreements, etc., and Ogle would put together the technical program and run it. Both would concern themselves with safety; each would keep the other continuously informed; and, of course, each could have input on any part of the problem. In essence it was to be a partnership with one (Starbird) being a little bigger partner than the other. By the end of November Rear Admiral Lloyd Mustin assumed command as Navy Deputy Commander and Brig. General John Samuel became the Air Force Deputy Commander.

The attempts continued to arrange a visit to Christmas Island to see if it was really satisfactory as a test site. At the November 2 DASA test coordination meeting, "The group was informed that there were no new developments concerning this island except that the British seemed to be dragging their feet on our request." Later there were several discussions of the subject in Washington between U.S. and U.K. government representatives, as well as a visit to England in mid-November. On November 16 the U.K. invited U.S. participation in a survey of Christmas Island, to be followed by briefings of senior U.K. officials before any further discussions between Macmillan and Kennedy. This invitation led to some confusion about U.S. members of the survey group which was settled with the appointment of Ogle as Task Force Scientific Deputy Commander. On November 22 Betts noted, "Arrangements for inspection tour of Christmas Island facilities expected to be completed very soon. AEC designees are Bill Ogle, LASL, and Pat Ryan, H&N. Understand that Ogle will represent both AEC and DOD." On November 27 Betts told Hertford:

> Arrangements for inspection tour for Christmas Island follow: Ogle and Ryan (Pat Ryan of H&N) should arrange for commercial air transportation to Hickam Air Force Base, Hawaii, reporting there at the Royal Air Force Liaison Office during the afternoon of December 4, 1961. Notification of DOD representatives selected expected on November 28. Current passports required. Headquarters, AEC, will notify British Embassy of security clearances for Ryan and Ogle. Clearance of U.K. representatives will be verified. Air Commander Whelan, RAF, and U.K. representatives Beards and Jones will join at Hickam. Travel beyond Hickam is via RAF air shuttle service, departing morning of December 5. Strict security required. For local consumption at Christmas and then only if necessary, the purpose of party on Christmas is in connection with survey work for possible use of the island in extension of satellite tracking facilities.

On November 29 Goeckermann sent to Ogle a list of items on which they wished information gathered during his upcoming trip to Christmas. These included topography features, hydrological features, existing structures and facilities, engineering details, support capabilities, weather data, industrial and radiological safety, administrative features, signal and communication cable and facilities, device handling and assembly, and transport and adaptability of site to the Livermore layout.

Samplers

The debate about the required samplers continued. As a result of the November 13 meeting in Albuquerque, AFSWC asked the Laboratories on November 17 about their requirements for collection of gaseous samples. Batzel answered on the 20th that LRL required gaseous samples on all LRL shots, that the gas sampling equipment should include "squeegee" compressors (not engine compressors) on all aircraft and should be the LRL-designed isokinetic flow wing probes used in Hardtack I on B-57-Bs, -Cs, and -Es. The B-57-Ds should have fuselage probes. On the 21st Graves commented that all the experience on diagnostic gas sampling was at Livermore, but that since Hardtack Phase I data had provided valuable diagnostic information, LASL concurred with any requirement for probes and gas sampling capability established by LRL. On November 20 AFSWC informed Systems Command: "This message outlines proposals for overseas atmospheric nuclear testing in spring of 1962 time period, and states requirement to be able to provide sampling of two shots per day on two successive days." In a meeting of LASL and AFSWC representatives on November 16 it had been agreed that in order to prevent cross contamination of samples and to preclude unacceptable radiation exposure to air crews and maintenance personnel, aircraft must not be reused within 72 hours to allow for decay of short half-life fission products and for physical decontamination of the aircraft. Therefore, six aircraft per shot, or a total of 24 aircraft, would be required, assuming 100 percent in-commission rate. The AFSWC message continued: "Because of expected yields, height of burst, and height of cloud in the most likely shot site, the fleet should consist of 14 B-57-B-type sampler and 10 B-57-D-type aircraft. In the event this number of B-57-D aircraft are not available, the total number should be kept at 24 by increasing the number of B-57 B-type samplers. AEC is proceeding to procure sampling tanks to equip a Pacific test sampling fleet of this size, resulting in an expenditure of approximately $500,000. Request you take action through Air Force channels to augment the B-57-B/D sampler aircraft capability, including modifications, air crews, maintenance personnel, and AGE in time to make good an overseas ready date of March 1, 1962. To ensure crew training and overseas movement, the increased sampling capability should be ready no later than January 15, 1962. Informal discussion with the 1211th Test Squadron indicates that they have a total of 19 B-57-B-type aircraft and 3 serviceable B-57-C aircraft on hand. Six to eight of the B-57-B types are committed to "crew cut" operations. This could require one to three additional B-57-Bs and seven B-57-Ds. In case of resumption of testing by other nations, additional samplers would be required if those detonations were to be monitored."

DOD Experimental Plans

DOD preparations for systems tests continued through November. In late October investigation of possible trajectories for the Atlas test had led to the conclusion that the Johnston Island area was not suitable as a target area, and Taongi Atoll had been suggested as an alternative. However, the political complications of involving a Trust Territory area precluded use of Taongi, and a new site was sought. On November 2 at the DASA Test Coordination Group meeting, the status of systems tests concepts was summarized as follows:

> Phase III, Atlas firing, can take place any time after October 30, without backup. This will be a Category III test. We have been told to try to fire beyond Wake with a short range for the missile. The plan calls for open water firing, 1,000 miles away from the test grounds. The Atlas will be fired from Vandenberg."

As for the ASROC test,

> The Operational Commander determined last night that he would go to sea and stand by and wait. Weapons are aboard the ships. Plans are complete as far as the Navy is concerned.

Planning for the Polaris test was just starting with no detailed operations order yet written. The submarine chosen was the Ethan Allen, and the shot area was to be about 350 miles southwest of Ascension Island. Four missiles had been designated and would have command destruct systems installed.

On November 3 Gilpatric notified the JCS that the Air Force and Navy efforts to prepare the ASROC, Polaris, and Atlas systems were to continue, but that the overall operational date was now no sooner than April 1, 1962. McNamara again confirmed to the JCS on November 9 that planning should continue for the three systems tests with planned execution dates within the three months after April 1. (As has been noted elsewhere, the Polaris and Atlas systems tests were deleted in the November 29 National Security Council meeting, but ASROC was left in as an effects test.) On November 10 General Gerrity of Ballistics Systems Division stressed to Systems Command the need for more positive thinking about Air Force needs for nuclear testing, expressing his feeling that weapon development tests were receiving the predominant consideration, whereas there was an urgent need for improved understanding of nuclear weapons effects, especially those involved in ballistic missile systems. By mid-November Sandia had designated a technical advisor for the ASROC and Atlas tests. On November 16 Gilpatric approved an additional 86 personnel billets for DASA, and on November 20 AFSWC noted their requirement for another 64 personnel in the Research, Development, and Test directorates since they seemed to be technically responsible for a major portion of the Air Force nuclear effects programs.

On November 16 and 17 the Bethe Panel met to review Russian progress. It was impressive. [redacted] These conclusions led Curtis LeMay, then Chief of Staff of the Air Force, to establish a committee to study the military implications of the Russian series of tests as interpreted by the Bethe Panel. He hoped to have the results in hand by January 5, 1962.

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More Political Considerations

New pressures to renew atmospheric testing, as noted earlier, had appeared during November. The Russians had declared that their series would end on October 31, but, apparently as a result of our announcement, Chairman Khrushchev, on November 5, commented that the U.S.S.R. was prepared to extend their nuclear test program if the United States resumed tests in the atmosphere. Nehru, at that time in the United States, stated that a test ban treaty was of the utmost importance, but, "As a formal treaty takes time, we insist on some kind of voluntary suspension to bridge the gap." On November 6 the U.N. General Assembly approved a resolution asking for a ban on all tests and urging the conclusion of a test ban agreement. In a sense as a reply to Khrushchev, Kennedy, in a news conference on November 8, emphasized that if the U.S. learned that Russia had made advances in understanding high-altitude nuclear effects, commensurate U.S. action must be taken. On November 8 the General Assembly adopted a U.S.-U.K. resolution proposing renewal of the Geneva test ban talks. On November 13 the United States proposed to the U.S.S.R. that the Geneva Conference be resumed on November 28, and on November 21 the Russians agreed.

Task Force Plans

The first steps along the path of technical consolidation of plans came in a meeting in Albuquerque on November 30, 1961. At that meeting Ogle explained the organization he and Starbird planned, pointing out in particular that while there would be military task groups , there would be no technical task group, only task units. Support services including construction, engineering, operations, and management were to be handled by Reeves, probably as Task Group 8.5. There was a review of the test program as it was then defined. (It had, of course, changed the day before, but the word had not gotten around yet.) The program discussed included four high-altitude shots from Johnston Island, probably using the Thor; eight airdrops and one, or possibly two, ground-based or ship-based shots for LASL; and eight to ten airdrops, two balloon shots, and one barge or ground shot for LRL. Both the Christmas Island and open sea operations were to be considered, and support requirements for Johnston, Maui, Midway, Kauai, and French Frigate Shoals were to be discussed. The report of the meeting sent to JTF-8 by Ogle is as follows:

> The following is intended to be an outline of requirements and arrangements as they now appear to me. A great proportion of these represent agreements reached at a meeting today in ALOO attended by representatives of LRL, LASL, Sandia, EG&G, H&N, ALOO, and DASA (Albuquerque). I would appreciate your passing these on to the Naval and Air Deputies and appropriate members of the staff, in particular J-3 and J-4 and the Task unit commanders.
>
> 1. As a manner of operating, the above organization will be considered task units with the following task unit commanders:
>
> LRL--Bob Goeckermann
> LASL--H. Hoerlin (temporary appointment)
> Sandia--D. Shuster (temporary)
>
> EG&G will not appear as a task unit at this time, but will instead satisfy the technical requirements of LRL and LASL, under the operational control of the support task group (Reeves). These units have now been told (by me) to submit operational plans and requirements to the task force J-3 (Ted Parsons) for coordination. All other requirements (construction, communication, etc.) will be submitted to the support task group (Sam Howell). After the consolidation of requirements, that task group will then take the appropriate action, i.e., pass on to the headquarters for action, or procure itself.
>
> 2. After due consideration, it becomes clear that the programs of the laboratories may now be broken down into several categories which may be treated separately, as follows:
>
> a. Airdrops: Of the 25 shots now proposed, some 15 to 20 will be airdrops. Some of the instrumentation of these shots serves both LASL and LRL, and one drop site is sufficient. It also appears that the equipment required is such that it can be packaged in trailers or vans which then may be used either on ships, on Christmas, or on Johnston. Until the use of Christmas is approved, we must prepare to use the ocean. Thus, a first requirement is for these instrumentation ships. A small carrier and two sea-plane tenders, such as the Curtis are suggested. The loading of these ships would be as follows:

CURTIS	CVE	CURTIS PRIME
Photo trailer (EG&G)	8 LRL diagnostic trailers	584 Radar (Sandia)
Timing trailer (EG&G)	584 Radar (Sandia)	Alpha (Sandia)
Two LASL trailers (time interval)	Alpha (Sandia)	Tracking (Sandia)
	Tracking (Sandia)	Photo (EG&G)
	Timing (EG&G)	Timing (EG&G)
	Photo (EG&G)	
4 trailers	13 trailers	5 trailers

> If Christmas is obtained, these trailers will then be used on land in three positions, and the diagnostic ships can be turned back. If Johnston is used, one, and possibly two, of the ships can be released. If we go to open sea, all are required. Since these ships should be loaded on the West Coast, I suggest that the latest date the ships should be available is February 1 on the West Coast. As a matter of backup, we should plan to use the presently instrumented C-130s and the instrumentation in the drop planes on all of these shots also. I should make it clear that the alternatives allowed above are not all equally desirable. In particular, because of accuracy and reliability, the technical fraternity would rate the comparative desirability of the several possibilities about as follows:
>
> Christmas--very good Johnston--moderate Open sea--poor

b. There are several diagnostic shots for which air dropping is not desirable for various reasons, as follows:

Withheld Under
5 U.S.C. 552(b)(3)
DOE, Exemption 3

(6) Spare "shot ships." We may need some extra floating shot points if there is trouble with some of the above shots, or if others appear. Perhaps 2 or 3 spares will do. A few comments on these shot ships may be in order. If we have Christmas, these shots would be fired at the same point as the airdrop target position. If we do not have Christmas, they will be done in the open sea using the three "diagnostic ships" for observation. Some of the diagnostics require space available only on ships the size of "Liberty Ships." Others could be done on smaller vessels, but in any case, they must be seaworthy. Anchoring systems for Christmas or sea anchors for open sea must be designed. Methods of getting people on and off in the open sea must be determined. I suggest that the Navy task group immediately get together with H&N (Sam Howell) to determine how these aims shall be accomplished. Some of the "shot ships" require considerable construction--collimators, vacuum pipes, assembly facilities, etc. The support task group is collecting the construction requirements for these, but that construction will probably have to be done in a shipyard somewhere, and time must be allowed to then get the ships to the shot point. They may have to be towed. Since some of the construction may be very time-consuming, a ship or so may have to be in the yard as early as January 1. Again, the Navy and support task groups should get together immediately on how to accomplish this construction.

c. Don Shuster is collecting, and will get to you very quickly, a set of statements similar to the above on the high-altitude atmospheric shots. However, some comments can be made now as follows:

(1) Assuming we use Johnston or that vicinity for firing the main missile, Sandia will fire instrument rockets from Midway, Kauai, and any island in the region of Christmas to which we have access for other purposes. These instrument rockets will be carriers for detectors furnished by LRL, LASL, and Sandia (and possibly DASA). Thus, any arrangements necessary for us to use Midway or Kauai for this purpose should begin soon. Some small construction may be necessary, but certainly trips by laboratory personnel to Midway and Kauai will be necessary very soon.

(2) Two ships which may be placed at intermediate positions as launching platforms for instrument rockets may be necessary. They would have to be more stable than, for instance, destroyers. I do not

suggest obtaining these ships now, because further thought on the experimental program may remove this requirement. However, it might be wise to keep this possible requirement in mind.

(3) Local timing signals on Johnston will be furnished in the normal fashion by EG&G. However, we must have an indication at Midway, Kauai, on the ships, and probably on Maui, that lift off has been achieved. This indicator should be accurate to a second or so. I suggest that J-3 get the armed forces communications people to satisfy this requirement. Obviously, if it turns out to be possible to fire the weapon missile at a prefixed time, then the time accuracy requirement of such a signal is reduced.

(4) Even without having the DOD requirements in our hands, it is clear that at least three instrumented aircraft for observation of the high-altitude shots are required. This instrumentation would observe photographically the expanding mass, take spectral measurements, observe cloud rise, etc. The instrumentation will be installed by LRL, LASL, Sandia--and I am sure the DOD will add some. Two planes would be close in, with the instruments looking almost vertically. One to observe cloud rise would be several hundred miles away. The main aim of these planes is to get above possible cloud layers. So it seems that KC-135s would be ideal, but if these cannot be obtained, C-130s might do, and for some purposes, even C-54s. I am sure the DASA will also put in a requirement for a plane or two to be at the conjugate point.

3. In summary, the requirements on us for special instrument carriers, etc., are in part as follows:

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<u>Naval</u>

a. Three diagnostic ships. Requirement may be reduced depending on method of operation.

b. Shot Ships

[redacted]

(5), (6), (7). Spares probably needed.

c. Targets--Radar reflector-carrying barges for drop plane to sight on. Discuss with Sam Howell and Air Task Group. May need an LSD or two for placement.

d. Instrument rocket ships. Possible requirement for two.

<u>Air</u>

a. Drop planes--it is to be noted that the requirement to be able to take off some devices from a remote field because of safety seems to have disappeared.

b. B-57 samplers.

c. Presently instrumented C-130s as backup for all airdrops. (Note that these planes are not configured to satisfy requirement on high-altitude shots.)

d. Instrumented planes (three) for high-altitude shots.

<u>Other</u>

a. U.S. shot island (Jarvis)

b. Long-distance time signal (Midway, etc.)

c. Permission to use Midway, Barking Sands (Kauai) for launching of instrument rockets.

4. Obviously, I have not attempted here to put together the more normal movements such as sample return, communications, transportation, etc. These will come to us through the normal channels.

Ogle, Shuster, Goeckermann, Strabala, Lieutenant Colonel C. R. Peterson of Field Command, DASA, Bill Adair of ALOO, and Bob Miller of ALOO discussed other aspects of the problem in a smaller meeting during the afternoon of November 30. (Don Shuster had by now agreed, after appreciable arm twisting by Ogle, to be the Assistant to the Scientific Deputy.) At that meeting, Ogle presented requirements that had been worked out between himself and Starbird, many of which are given in Tables XXXII through XXXVI).

TABLE XXXII
PROPOSED OVERSEAS SHOT PROGRAM
(Alternate Plan)

Assumptions: (1) Detonations begin April 1, 1962; complete June 30, 1962
(2) Limitations: 23 shots total
(3) Locations:
(a) High altitude-Johnston Island
(b) Off Johnston Island
(c) One or two shots off small island not yet identified

<u>General Events and Sponsors</u>

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5 U.S.C. 552 (b)(3)
DOE, EXEMPTION 3

Further assignments were made. The AEC (Reeves), through EG&G, would be responsible for close-in, ground-to-ground, and timing signals. If feasible, the AEC would retain a ship-to-shore and long-range signal system, but would concede if necessary to the DOD. The AEC would assume responsibility for radiological safety, utilizing REECo. Holmes & Narver would collect the requirements. Bill Sanders would be responsible for support and any other duty agreed upon. ALOO would be responsible for construction and Bob Miller would have responsibility for planning and coordination and liaison with JTF, particularly with Ogle and Shuster. Pending the formal announcement of JTF-8 establishment, criteria would be furnished to H&N directly from the task units, and operational requirements would be forwarded directly to Colonel Parsons, JTF-8 Deputy for Operations.

On the same day, November 30, at Vandenberg Air Force Base, representatives of AEC, Douglas Aircraft, Sandia Corporation, and H&N discussed ground facilities required for the Thor missiles at Johnston Island. Determinations were made concerning a similar launch facility already at Johnston Island, and initial criteria were presented for shop facilities and other support of the launch facility. H&N was authorized to provide a survey crew and to make "as-built" surveys of critical areas.

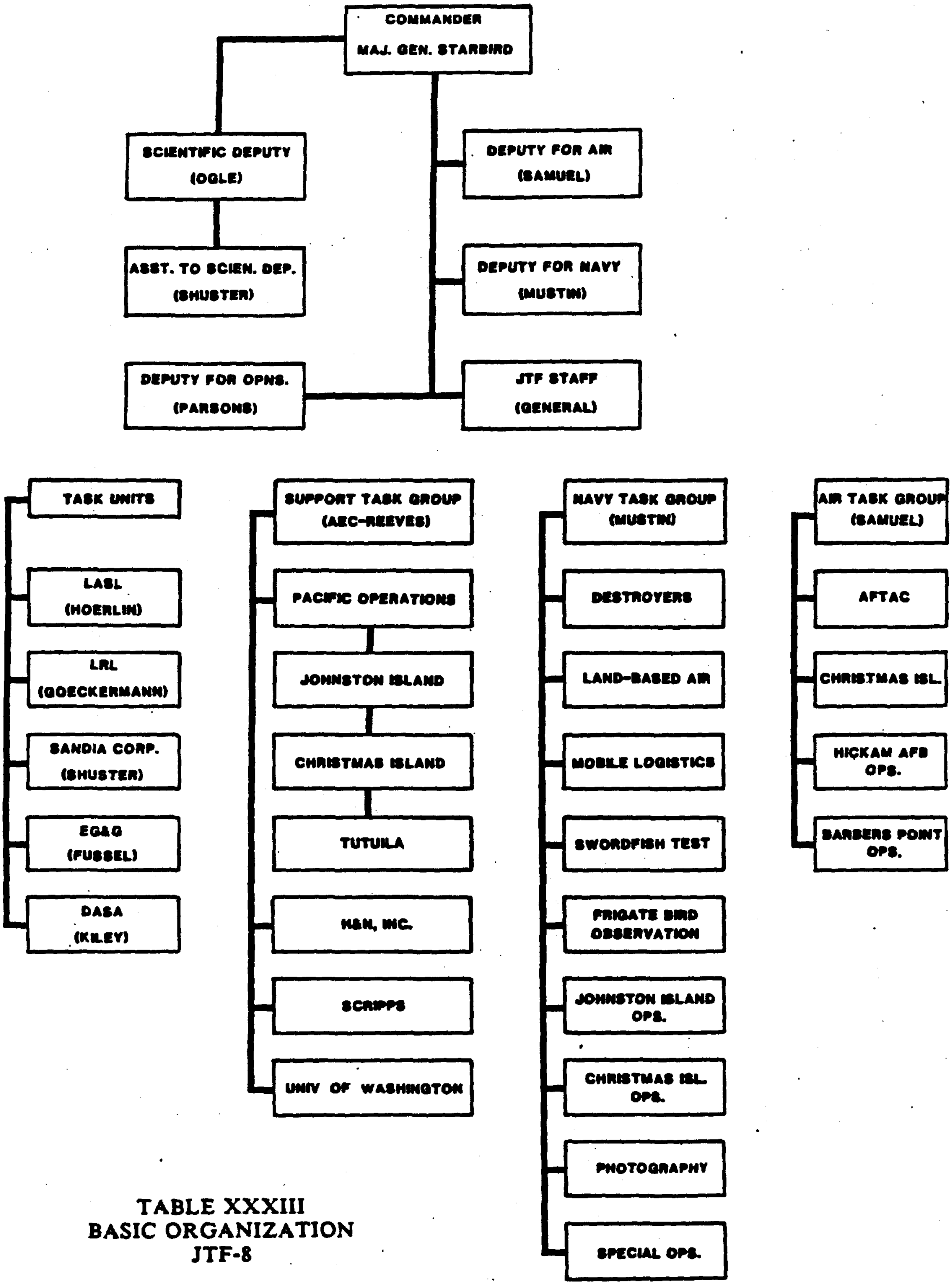

TABLE XXXIII
BASIC ORGANIZATION
JTF-8

5 U.S.C. 552(b)(3) DOE, EXEMPTION 3

TABLE XXXIV
SHIP AND OTHER SEA REQUIREMENTS

Three Diagnostic Ships
- U.S.S. Curtis
- CVE
- U.S.S. Albemarle

Five Target Ships (Liberty Type)

[redacted] Propose to find small island; however, could detonate on ship.

Range Stations:	2 Ships for Intermediate Range Stations
Rocket Ships:	2 Rocket Ships CCVE or Equivalent
Target Barges:	30
Air-Sea:	Nose Cone Recovery Capability for High-Altitude Shots

TABLE XXXV
TRAILER LOADING OF SHIPS

U.S.S. Curtis	CVE	U.S.S. Albemarle
1 Trailer Photo (EG&G)	8 Trailers (LRL)	1 Trailer Radar 584 (SC)
1 Trailer Timing (EG&G)	1 Trailer Radar 584 (SC)	1 Trailer Alpha (EG&G)
1 Trailer (LASL)	1 Trailer Alpha (EG&G)	1 Trailer Tracking (SC)
1 Trailer (EM) (LASL)	1 Trailer Tracking Mount (SC)	1 Trailer Photo (EG&G)
	1 Trailer Photo (EG&G)	1 Trailer Timing (EG&G)
	1 Trailer Timing (EG&G)	

Note: H&N to be responsible for furnishing power supply of technical programs aboard ships.

TABLE XXXVI
LAND USE REQUIREMENTS FOR HIGH-ALTITUDE SHOTS

Maui:	Camera shelter--12 cameras; 3 spectrographs; weather.
Kauai:	20 cameras; documentary photo, rocket firings (50 people).
Midway:	4 cameras; documentary photo (10 people).
Johnston:	Rebuild Hardtack II facilities; photostations, launch pad, 4 rocket launchers (75-100 technical people and support).
French Frigate:	Photo (5 people).
Palmyra:	Additional rocket sites probable.
Christmas:	Additional rocket sites probable.

AIR REQUIREMENTS

Drop aircraft plus 2 C-130s required for all shots. (Drop aircraft cameras may be used as backup on high-altitude shots.)

B-57 Samplers: Now estimated at 6 operational including controller; based upon sampling 2 shots on 2 successive days at 2 geographically separated locations.

2 high-altitude (above cloud) aircraft. (Mission not defined.)

~~SECRET~~

The Acquisition of Christmas Island

On November 29, 1961, the proposed visit to Christmas Island was approved. Ogle and Ryan were authorized to discuss "restricted data" with U.K. personnel only if necessary to accomplish the purpose of the visit. Very little could be said about the upcoming program and any "restricted data" discussed was to be reported after the trip. Armed with a mass of questions from Goeckermann, the party left Hawaii at 9:15 a.m. on December 5, 1961. The members of the party were Air Commodore J. R. Whelan, RAF; Colonel Carmel M. Shock, AFSWC; Mr. W. E. Jones, AWRE; J. P. Ryan, Holmes & Narver; H. L. Beards, U.K. Ministry of Aviation; and William Ogle, LASL. The British members had been briefed thoroughly on the purpose of the trip and were authorized to discuss possible ramifications of any agreement. [redacted]

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1.3 (a)(6)

In general, the technical facilities were either in poor condition or inadequate, but some things were good. The airplane decontamination area looked promising and the radchem lab was in usable condition. The photo lab appeared to be large enough. Many buildings near the region of the airfield would be useful for labs and offices, if required, although minor rehabilitation, interior painting, and wiring checks would be needed. The forward area was not in good shape, there being a few very small buildings that might be useful. The balloon site was in good shape. Profiles of the ocean bottom had not been run along the southern coast where barges might be anchored, and no measurements of the ocean currents had been made. The channel into the harbor at the Port of London could accommodate LCMs, and probably LCUs, but nothing larger, necessitating lightering for some material. The island could not, at the time, house appreciably more people than were already there. The main camp, which was designed for 2,500 to 3,000 people, looked as if it could be rehabilitated with only moderate effort, but cooking facilities were questionable. The 50-cycle electrical power would be a problem for U.S. equipment. Water was clearly not available in sufficient quantities; additional distillation equipment would be needed. The roads were adequate, but the British drive on the wrong side of the road. There were many light vehicles, but all were British gear. Fuel movement would be a problem. There was a 60-bed hospital, but only the operating room was maintained.

Possible assistance from the British was discussed. They commented that they could house and support perhaps 50 people for a few weeks while we were getting started, but it would be a big strain on them. They offered knowledgeable people to advise us on details of their setup, how their equipment works, etc. The equipment they had, such as trucks and jeeps, would be available to us. A mutual arrangement would have to be made for replacement or pay or whatever. They would operate, or help operate, the airfield control tower, and their technical people would be interested in making some measurements for us if it were desirable. The British said that we should expect very little off-site fallout from airdrops or balloon shots,

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since, during Grapple, they had made fallout observations at Fanning, Malden, Kwajalein, Fiji, Aitutaki, Canton, Samoa, Penrhyn, Honolulu, and Rarotonga, and nothing of note was observed. Typically, the wind was out of the north and there were two layers of scattered clouds, one in the region of 6,000 to 7,000 feet and the other between 2,000 and 3,000 feet. They commented that placing airdrops in a large enough hole in the clouds to take satisfactory pictures would hardly ever be a problem.

There was also some discussion of preliminary concepts of operations. The British would require that coconut plantations not be contaminated or damaged in any way, which was no problem since the same ground rules would be followed to protect our own camp and operations center. There must not be any remaining radioactive debris that would be a real hazard to the natives after the tests were done. As a consequence of these rules, tower or surface shots might be very questionable. The British would probably insist on their vetoing our firing if they judged the winds to be improper. Contamination of the native fish supply did not appear likely from barge shots off the south end of the island because of the sea currents, but monitoring would be necessary and native help should not be sought. Normal activities of the native populace must not be disturbed except when shots were actually being fired, and the subject of compensating the native workers for lost time would have to be discussed. The native villages, etc., would be off limits to our testing personnel, and commercial arrangements, either with individuals or organizations, should be made with the Gilbertese people. It was further noted that there were accommodations for only two women on the island. The conclusion of Ogle's trip report is worth noting:

> While Christmas Island is not developed to the extent that is Eniwetok/Bikini, it could be made into an eminently satisfactory site for atmospheric tests. The main point that strikes the observer immediately is that there is so much space, all flat. Airfields, parking ramps, etc., can be as large as necessary. Buildings need not be crowded together, scientific stations can be properly placed. There is no serious fallout hazard. The weather is good. The site seems to be ideal for balloon sites and airdrops. It is more difficult for barge shots because of deep anchorage, but experience would probably teach us how to do even this properly. While there are many problems, it appears that the most serious ones that arise in considering a quick operation have to do with the technical facilities, and particularly those concerned with alpha. For longer-range planning, the main problem is clearly that of docking facilities for large ships. Therefore, from an operational and technical point of view, Christmas Island is to be highly recommended. Politically, of course, the finger may point elsewhere.

In preparation for a joint U.S./U.K. meeting after the survey trip the AEC approved the following guidelines for the U.S. representatives who would attend:

a. The U.K. representatives shall be informed that the decision to test or not to test in the atmosphere is in no way contingent upon the availability of Christmas Island; rather, the use of Christmas Island would facilitate and improve the test program.

b. It will be appropriate for the American representatives to make available to the British the sort of information contained in the letter of November 29 from Chairman Seaborg to the President. (Ed. note: The NSC subcommittee letter giving the proposed program.)

c. The American representatives can agree that the results of individual tests involving the use of Christmas Island will be made available to the U.K.

5 U.S.C. 552(b)(1) 1.3(a)(3) DOE, Exemption 1

On December 9 in a memo to the Secretary of State, Phil Farley noted that both the Prime Minister and Foreign Secretary of England would have to be convinced "that our proposed tests are necessary to maintain a free world security and that a sound and consistent public defense of resumption of atmospheric tests can be made in the U.S. and the U.K." He noted that by sending the reconnaissance party to visit Christmas during the week of December 4 and arranging for the review with senior U.K. technical officials of the testing program on December 8, [redacted] He thus suggested that the matter be discussed in Paris with Lord Home, specifically noting that the President had not decided to resume testing, that our tentative test program followed the criteria given by the President and Prime Minister, that we were strongly interested in the use of Christmas Island in view of the undesirability of reactivating Eniwetok, and that we should attempt to ascertain any specific difficulties which the British see beyond those raised in the Prime Minister's letter of November 16.

On December 14 John Foster told Seaborg that, "We continue to feel that Christmas Island can represent the most desirable test location for the atmospheric series if it can be made available for exclusive use by the U.S. from January 1 through July 1, 1962 . . . ." On the same day Al Graves indicated to Betts that LASL fully supported the use of an island such as Christmas.

On December 15 AEC Commissioner Haworth wrote to McGeorge Bundy, the President's Special Assistant for National Security Affairs:

> To summarize, the availability of Christmas Island by January 1962 for the coming series of tests would be highly advantageous in that it would permit the conduct of a more extensive, more carefully instrumented, and operationally simpler program with greater assurances of attainment of test objectives.

He pointed out that if too rigorous restrictions were imposed by the British, for example, on our freedom of operations control, these advantages could be nullified.

On December 21 President Kennedy discussed the use of Christmas with Harold Macmillan in Bermuda. Kennedy pointed out that the U.S. needed British support in any decision to test in the atmosphere and that the British colony of Christmas Island in the central Pacific offered an ideal site for testing in the atmosphere. He asked whether Macmillan would agree to atmospheric tests on Christmas Island if the political situation did not change vis-a-vis Russia, and Macmillan stated that that was a decision for the Cabinet, but noted that Britain and America were partners and we were in this together.*

In other discussions at lower levels during the Bermuda meeting a tentative agreement was reached governing the use of Christmas if it should become available to the U.S. The agreement stated that the island would be used only in conjunction with a test program of an agreed-upon general nature and purposes. Only airdrops or balloon shots would be used. The U.S. would have responsibility for control of the various aspects of the tests, including their selection, scheduling, timing, and the application of safety rules. However, the U.K. would have a base commander who would be a member of the safety committee. The U.S. could construct buildings and facilities as it deemed necessary at its own expense, but approval of major facilities and buildings should come from the U.K. base commander. The U.K. would assist in providing security protection at Christmas Island. The U.S. would, in accordance with

*A. Schlesinger, *A Thousand Days*, page 491.

existing agreements for cooperation (JOWOGs), furnish or otherwise make available to the U.K. detailed information concerning the tests done from Christmas Island. The U.S. would be responsible for handling loss and damage claims following such tests. All arrangements would be made without prejudice to either nation's claims to sovereignty over Christmas Island. On December 27 Phil Farley asked General Betts and Gerry Johnson to review the draft statement of principles.

Early in January 1962 Macmillan, in expressing to Kennedy his unhappiness at the thought of test resumption, noted with strange irony that he should have spent Christmas Day wondering how to commend to his cabinet colleagues the dedication of Christmas Island for this purpose.* In further discussion, he also suggested that the three leaders try once more for general disarmament and a test ban, noting that the forthcoming March meeting of the 18 power disarmament conference in Geneva would be appropriate for this purpose. He did not indicate whether, in his view, the use of Christmas Island was conditioned on U.S. agreement to a disarmament conference at the Summit or whether his agreement to the resumption of American atmospheric testing could come only if the conference failed. On January 12 Rusk suggested that Kennedy's reply should reject any link between the use of Christmas Island and a new disarmament initiative.

On January 17 Luedecke (AEC) sent Phil Farley the results of the DMA and MLC reviews of the draft statement of principles. It was noted that in addition to using Christmas Island as a test site, the AEC felt it important to add that the airfield and other logistics support facilities would be needed for test activities away from Christmas Island. The AEC wished to suggest that the statement of agreement not preclude firing from barges or other types of shots carried out some distance from the island. The preferred interpretation would be that only airdrops or balloon shots would occur near the island. By January 18 the test planners were nervous, and Betts, noting that the island would probably not be secured for AEC use until about February 1 at the earliest, asked the Laboratories if they could still prepare a meaningful experiment to give reliable data within the proposed time scales.

On January 28 Starbird told Ogle that he might describe in detail for the chairmen of the AEC and the MLC the impact on JTF-8 plans of further delay in the Christmas Island decision. In essence he felt that even if the British agreed now we could not use the island because it would take four to five months to prepare it for a major operation; since we were planning to use Jarvis or some other isolated island for a surface shot and had hoped to use Christmas for sampler aircraft operations, we were in trouble on that too; we were making modifications at Johnston Island for sampler operations without intending to operate them from that base, but if we didn't have Christmas we would have to use Johnston. Therefore, he intended to recommend to the Secretary of Defense that the U.S. immediately indicate to the British that we were no longer interested in near-term use of Christmas Island for a major test series, but we should indicate our desire to use it for support of open sea activities. He commented that if we delayed past April 1 even this last possibility might be out. Ogle responded by telephone and TWX, commenting, "Four or five months to get Christmas ready seems long to me--technical end could still be done in remaining time, but difficulty would come in getting camp support, airfield support in time."

On February 2, 1962, Bundy told Betts that on February 8 the British would agree to our use of Christmas Island. As a result Starbird planned to visit Christmas Island starting February 9, but on February 7 the U.K. representatives informed the U.S. State Department that they were adamant that preparations not begin at Christmas

*A. Schlesinger, *A Thousand Days*, page 492.

Island until the intergovernmental agreement was signed and that they would not agree to Starbird's planned visit. They did agree that their Assistant Secretary Anderson (Atomic Energy) and Air Vice Marshal McKinley would fly to Washington on the 9th to meet with Starbird, and that after those discussions they would be prepared to fly with him to Christmas Island if such a trip appeared advisable. They promised that the United Kingdom would react to the State Department's comments on the proposed island agreement by the end of the week, i.e., by February 9. On February 8 the White House issued a statement that the U.K. had agreed to permit the use of Christmas Island by the U.S. for nuclear tests.

The final agreement (with interpretation of certain provisions) for U.S. use follows:

MEMORANDUM OF UNDERSTANDING COVERING ADMINISTRATIVE, FINANCIAL AND SCIENTIFIC COLLABORATION ARRANGEMENTS FOR THE USE OF CHRISTMAS ISLAND BY THE UNITED STATES GOVERNMENT IN CONNECTION WITH THE PROGRAMME OF NUCLEAR TESTS DISCUSSED BY THE PRESIDENT AND PRIME MINISTER AT BERMUDA DECEMBER, 1961

Withheld Under
5 U.S.C. 552 (b)(1)
1.3 (a)(3)
DOE, EXEMPTION 1

Withheld Under
5 U.S.C. 552 (b)(1)
1.3 (a) (3)
DOE, EXEMPTION 1

Withheld Under
5 U.S.C. 552(b)(1)
1.3 (a) (3)
DOE, EXEMPTION 1

Withheld Under
5 U.S.C. 552 (b)(1)
1.3(a)(3)
DOE, EXEMPTION 1

Withheld Under
5 U.S.C. 552 (b) (1)
1.3 (a) (3), EXEMPTION 1

Withheld Under
5 U.S.C. 552 (b)(1)
1.3 (a) (3)
DOE, EXEMPTION 1

Withheld Under
5 U.S.C. 552 (b)(1)
1.3 (a)(3)
DOE, EXEMPTION 1

Withheld Under

5 U.S.C. 552 (b)(1)

1.3 (a) (3)

DOE, EXEMPTION 1

Withheld Under
5 U.S.C. 552 (b) (1)
1.3 (a)(3)
DOE, EXEMPTION 1

Withheld Under
5 U.S.C. 552 (b)(1)
1.3 (a) (3)
DOE, EXEMPTION 1

The High-Altitude Carrier

On November 19, 1961, AFSWC published a report which included a complete outline of the high-altitude program. The report assumed three tests named Bluegill, Kingfish, and Starfish and, on the basis of nuclear safety, eyeburn, and operational suitability, concluded that Johnston Island (others considered were Eniwetok, Kwajalein, Christmas Island, and Vandenberg AFB) was the most promising operational base for the tests. From consideration of boosters available promptly, and noting that Sandia had already accomplished the detailed study of mounting appropriate warhead devices in the Thor reentry vehicle, they stated that "since these warheads fit the yield requirements, they were the logical choices for consideration and the Thor then became a 'first choice for the operation.'" The AFSWC study had also considered the Polaris, Redstone, and Blue Scout missiles. The Redstone, it was noted, did not have the required altitude capability and would require extensive modification of the warhead fusing system. The Polaris could meet the altitude requirements, but would not easily accept all of the warheads and had no provision for attaching external instrumentation packages. However, the operational flexibility of the Polaris was seen as an extremely desirable feature. As for the Thor, it was stated:

> The Thor booster is available from current inventories, can accomplish all altitude requirements, requires only minor modification for adaptation to the proposed warheads, and has an established high degree of reliability. Twenty-three out of twenty-five Thor space boosters launched since October 4, 1960, have been successful. The overall space booster success is 55 out of 62 launches. The Thor also has provisions for installation of external ejectable scientific instrumentation packages.

The conclusion was that the Thor from Johnston Island was the most acceptable combination for Project Fishbowl.* To provide close-in measurement capability, it was proposed to install on the exterior of the vehicle, at the base, three ejectable scientific instrumentation pods. Additional instrumentation would be positioned by using sounding rockets. The overall cost for the three-shot program (including one spare booster) was estimated to be about $40 million, exclusive of JTF-8 costs, and the preparation time was estimated to be five months under the most accelerated, high-priority conditions. "Limiting items appear to be payload design and fabrication, procurement and installations, and the training of sufficient launch crews to satisfy the small vehicle program." The report further detailed the small rockets required, showed schedules for the total program, and listed desirable experiments to be fielded.

Even before Starbird assumed command of JTF-8 in Washington he reviewed the recommendations for carriers for the high-altitude shots. Both AFSWC and DASA Field Command had recommended the Thor. The use of the Thor would require the use of Johnston Island, although Johnston might be required anyway as a base for launching sounding rockets. During the last week of November and the first week of December Starbird asked the Military Services to propose warhead carriers. The Air Force, together with Douglas representatives, proposed the Thor, and the Navy was quite enthusiastic about using Polaris, pointing out that it might be possible to use the ship "Observation Island" in order to launch from the ocean surface. On the other hand, the Army was less than enthusiastic about the Redstone, which was now an old missile, and the Nike-Hercules, but did point out they were available and could be used. Starbird's outlook, as transmitted to Booth with his final recommendation, is paraphrased below. He had investigated only the three systems, Polaris, Thor, and Redstone, and considered eight questions as follows:

1. Is the booster one which has been proven to be reliable? There is little difference in reliability of the three systems in delivering a payload to a satisfactory position in space, with Redstone having the best record and Thor slightly behind, although both were better than 90 percent; Polaris, although having a lower probability of successful performance, was a newer booster and had remarkable success in its short period of limited firings. (Ed.note: Ogle pointed out to Starbird that, from his point of view, both Redstone missiles had failed in the Teak and Orange tests of Hardtack.)

2. Will a trained team be available to conduct the firings? The Navy would organize experienced personnel into a cadre for a full ship missile team which could be ready in early May. The Air Force proposed to use an experienced contractor team to assemble the equipment, make preshipment checks, perform the installation at Johnston Island, and perform the firings. The Army would assemble immediately an experienced crew. None of the Services proposed to furnish a crew which had been functioning recently as a team, but each could provide a satisfactory team by the proposed date.

3. What data-gathering capability would be incorporated in the missile? The Navy plans would incorporate four powered pods in the nose section, including one nose ejection pod. The Army would use unpowered tail pods as done on Hardtack and would also design and build a nose ejection pod. The Air Force would use unpowered Atlas pods on the tail section and would not have a nose ejection pod.

*The DOD participation in the high-altitude tests of Operation Dominic.

They proposed to position certain items by supplemental rockets. (Of course, the Navy system had not yet been designed, whereas the Air Force had done appreciable work on the proposed Thor pods.)

4. Is any critical engineering and development required for each proposal? Here there is some notable difference between the boosters. The Polaris would require some modifications and new designs: the nose cone shape would be new; the warhead adapter and firing system for the ship would have to be designed and built; and powered pods were a new requirement. Although Navy studies indicated no problems in any of these efforts, the schedule necessary to accomplish two shots by June 15 left no room to remedy unexpected difficulties. For the Redstone, the nose ejection pod and the warhead adapter kit must both be built. The firing and fusing system used on Hardtack would be used again, with some modification still to be designed. As for the Thor, no significant modifications of the warhead nose cone configuration or existing adapter kits would be required. A new firing and fusing system would have to be developed and the Atlas tail pods have been flown on the Thor. In summary, for this question, "It appears that significant engineering and development is required for the Polaris system. That needed by the Redstone is less by considerable degree, and that for the Thor still less, although, in the Thor case, it will be centered around the critical firing and fusing elements." (Sandia had already started working on the firing and fusing systems.)

5. What systems test is possible prior to nuclear testing? Only for the Polaris is a prior systems test proposed by the Services. A full Polaris test with a ship missile crew system would not occur before May 1 and, at that, very little time would remain to remedy any gross deficiencies. As for the Thor, the time required to prepare the fusing and firing set and incorporate the tail pods should allow conducting the test from Vandenberg AFB within 2 or 3 months. A Thor systems test for Johnston Island could not occur probably before mid-May. As for the Redstone, a limited systems test incorporating the nose pod and fusing and firing systems changes could be done at Johnston early in April.

6. Does the system have adequate technical flexibility? Providing the Navy's schedule can be met, the Polaris has by far the most flexible system, which allows firing from the ship and counting down two missiles simultaneously to T minus 1 minute and holding there indefinitely. Additional shots could be performed with minimum time delay and no fixed land base would be required. The Thor can be counted down to about T minus 8 minutes and held there due to the short fueling time required. The Redstone, on the other hand, begins to be fueled at T minus 45 minutes and only holds for periods up to 3 or 4 hours. Further difficulties arise after T minus 15 minutes when the batteries must be replaced if an extended hold is required. As for other considerations of flexibility, the Redstone is limited to about 800 kilometers altitude, whereas the Polaris and Thor can get well over 1,000 kilometers. All three boosters thus meet current requirements, although the possibility of a test at altitudes over 800 kilometers would provide an obvious problem. In summary, "The operational flexibility of being able to operate from any chosen area on certain notice gives to the Polaris a definite advantage over the other two systems. The simultaneous countdown feature and the long T minus 1 minute hold capability are also great assets. Of the liquid fueled systems, the Thor's longer hold capability at T minus 8 also gives a significant advantage over the Redstone. Yet any of the three should be capable of sufficient flexibility to permit the firing to

occur under opportune weather conditions and in coordination with other instrumentation."

7. Does each system give assurance of being able to accomplish the required program within available time? Assuming the Bluegill and Starfish events and a final cutoff of July 1, the final shot should be planned to be done by June 15 in order to take into account delays. The Polaris, requiring ship conversion and a payload redesign, could be scheduled for test early in May, followed by the nuclear shots on June 1 and 15. However, unforeseen engineering, development, or ship conversion delays could retard these dates and it would not be possible to advance either of the firing dates without foregoing the proposed systems tests. The Thor program, which would include a Vandenberg shot, should be capable of executing the two tests on May 15 and 30, providing some time cushion. The Redstone would permit the greatest cushion, with a certification test at Johnston on April 1 and perhaps nuclear tests 15 days thereafter.

8. Does each system give assurance against catastrophe and personal injury? None of the three systems sponsors has yet provided an overall safety analysis or submitted complete hardware designs. The proposed warheads are one-point safe. "As of now, no one of the three systems would appear to be ruled out by a lesser chance to give sufficient protection against premature nuclear detonation or nuclear contamination."

Based on his study, Starbird then recommended the Thor, assuming that a systems test at Vandenberg would be successful. His primary reason for selecting Thor over the Polaris was that it gave greater assurance of conducting the planned firings within the period allowed. His primary reason for recommending Thor over Redstone was the Thor's higher-altitude capability and his belief that we might want to fire the 1,000-kilometer or higher shot during or immediately after the series. Starbird also commented that it was his intent to "assign a special assistant to the Scientific Deputy who will have as his sole responsibility coordination of the high altitude program." Eventually Don Shuster accepted that responsibility. Starbird sent the above recommendation to Booth on December 7 and Booth informed Air Force Headquarters of the decision on December 15, 1961. DASA further requested of the Air Force a propelled pod from the nose for Bluegill and three other pods for each shot.

The decision to use Thor clearly settled the question of the launch site, which would be Johnston Island.

General Observations

During the months of December 1961 and January 1962 the organizations were firmed up, detailed operational and experimental plans were made, and procurement of equipment, ships, airplanes, etc., was started, all in parallel. While the organization continued to change to a certain extent throughout the operation, the Task Force organization was pretty well settled by the end of January. By early December Task Force Headquarters had obtained the use of Barton Hall in Washington. In mid-December U.S. Army Colonel Roger Ray was assigned as a Deputy to Ogle to concern himself with the test device carrier missiles. In late December H&N appointed Paul Spain as construction coordinator for the overseas operation; this was the beginning of the "Spain Committee," which consisted of one member from each Laboratory, EG&G,

Field Command, JTF-8, and the AEC. It was quickly agreed that all construction requirements would be sent to the Spain Committee, who would coordinate these requirements, check for necessity, and arrange that the requirements be fulfilled. Jim Sugden (H&N) was assigned the responsibility for coordinating communications requirements, and EG&G (Frank Strabala) was given the job of collecting requirements and supplying firing signals. Within the Laboratories, similar assignments were quickly made. The LRL appointments have already been noted. For LASL Lee Aamodt took on the job of heading the Task Unit, with Herman Hoerlin as alternate, basically for high-altitude efforts, and later with Austin McGuire as alternate. Shuster was initially responsible for the Sandia Task Unit, but after designation as Deputy to the Scientific Deputy he turned the Task Unit over to others. Frank Strabala ran the EG&G organization.

Thus, through December and January the operational concepts became clearer. The high-altitude operation would clearly be done from Johnston Island. The AEC development program would be done mostly at open sea, either with airdrops or surface detonations, and including one island shot. The ASROC effects test was also part of the program.

The Open Sea Operation

While awaiting a decision on Christmas Island, the AEC Laboratories and the Task Force had no choice but to plan for open sea detonations. The aborted Operation Everready had established a concept which was, somewhat reluctantly, developed further by the testing organization. However, all experimental plans were made and equipment was obtained with the idea of being able to move to Christmas Island if that facility should become available. The concept was hammered out in dozens of meetings during December and January. The intent was to do either airdrops or shots with devices emplaced on Liberty ships, which would, of course, be blown up. For an airdrop there would be a free-floating "target" raft, 20 by 24 feet, outfitted with radar reflectors, lights, and radar beacons. In addition there would be an air array consisting of the B-52 drop aircraft (two were available), the two C-130 diagnostics aircraft that had been obtained for Operation Everready, and a C-121 air array control plane which would be backed up by control from an aircraft carrier. At an appropriate distance from the target would be the command and control ship (the carrier Hornet) and two diagnostics ships (McGraw and Merrill), which were MSTS C2 ships with helicopter pads. On each ship two radars were available for tracking; the ship's radar and one installed by Sandia (584 and GMD radar). A DME system was also provided to determine distances from the ship to the target raft and the bomb. JTF-8 would have its command post on the Hornet.

P2V aircraft stationed at Barbers Point NAS (Hawaii) would be used to clear the test area ahead of time, assisted by two destroyers which also served as weather ships. Additional weather information would be obtained using WB-50 aircraft. B-57 samplers, the B-52 drop aircraft, and the C-130 diagnostic aircraft would also be based at Barbers Point. Other aircraft involved included air/sea rescue and C-135 sample-return planes, all of which would be based at Hickam AFB.

The danger area was to be a 400- by 600-mile area, with its near edge 300 miles south of Oahu, although on occasion it was argued that the boundary could be as close as 100 miles from Oahu. The 300-mile minimum distance from Oahu was, on occasion, somewhat disturbing because the B-57-B's operating radius was only 434 nautical miles, meaning that they had very little sampling time, especially if the detonation were farther on into the danger area.

For an airdrop, the LASL devices would be dropped from 45,000 feet in free fall, in either Mark 39 or Mark 15 cases, whereas the LRL devices would use Mark 36 cases with drogue parachutes, which would be dropped from an altitude between 25,000 and 35,000 feet. Primary diagnostics were based on the MSTS ships. Fireball cameras were mounted on EG&G-designed tracking platforms which were operated from the ships fire control system. Both LASL and Livermore had optical and EM time interval measuring gear on the ships and on the C-130s, and LRL intended to install additional optical equipment and appropriate EM gear on the Hornet. LASL planned to make alpha measurements on airdrops utilizing an instrumentation drop case together with the device drop case. For this measurement the Mark 28 instrumentation drop case containing alpha detectors and appropriate telemetry gear would be released from the B-52 at an appropriate time before release of the bomb so that the instruments were at the proper distance from the bomb when it detonated, the proper distance being based on the correct intensity range for such a measurement. Data would be telemetered from the instrument case to one of the MSTS ships and recorded by Sandia gear. The B-52 would also be equipped with fireball cameras and bhangmeters. The radar reflectors, lights, and beacons to be used on target rafts were designed and procured by AFSWC with AEC funding and were to be installed on Navy rafts. The Navy would then have the responsibility for proper target placement.

General Samuel directed that all bombing should be done by radar with visual backup, but Ogle was arguing in February that it should be visual bombing with a radar backup because of the previous experience in Nevada.

The ship array would be gently under way at shot time on a heading of 270°; however, that point was still being argued at the end of January since 270° put the ship abeam of swells and that could be very uncomfortable. However, the Laboratories wanted the 270° orientation since the shots would be fired early in the morning and they wanted the optical gear to be looking (westward) into a dark sky to achieve maximum contrast.

There were several hazards to worry about. The B-57 maximum range has already been mentioned. Obviously, shot time would have to be chosen so that attendant weather conditions would not result in a fallout hazard to Hawaii, either for a normal drop or for an accidental detonation on the surface. Starbird worried some about the latter point and suggested a safety link from the bomb that would prevent surface detonation, but Ogle estimated that with the present system, the odds of a surface detonation were about 1 in 10,000, and the safety link would degrade the reliability of the fusing system. Such a link was not used. It was estimated that if the ships were 10 to 20 miles away, then even in the case of a surface burst the base surge radioactivity would not be hazardous. Based on Eniwetok experience, Ogle estimated that the ships would be safe from blast damage if they were six miles from a shot of 100 kilotons yield, 10 miles from 1 megaton, 15 miles from 3 megatons, or 20 miles from 10 megatons. These distances were somewhat conservative compared to later Navy-produced numbers. Because of the possible tsunami problem associated with the tests, Bill Van Dorn of Scripps Institute was asked to help, not only with that general problem for the airdrops, but for any problem that arose in the operation.

The Laboratories initially considered firing several bombs on Liberty ships using a radio link from the command ship. Some six Liberty ships were requested, but by the end of December LASL had withdrawn their request for three of the ships. Livermore continued their request, and on January 23 the Willy Jones arrived in Pearl Harbor to undergo modifications for the Livermore Lute shot. For such a shot the device ship either had to be anchored in deep water or had to have very heavy and deep sea anchors. H&N and the Navy, early in January, set about trying to obtain such anchors.

The Hornet and the MSTS ships were made available for modification on the west coast in mid-January. On January 19 parallel work on engineering design and modifications began on the Hornet with the intention of putting equipment aboard by February 12, ready or not, in order to meet a first dry run date of February 28. The ships were scheduled to leave the west coast on March 6, arriving at Pearl Harbor on March 12 for any last-minute changes, and leaving on March 22 for practice runs.

Thus, most of the gear for this kind of operation was under construction or in hand and being installed on the ships by early February. However, the President announced on February 8 that arrangements had been made with the British to use Christmas Island. Work continued for the next week on ship modification and equipment installation while a decision was being made on whether or not to move to Christmas. The Lute shot had been canceled late in January, and, hence, work on the vessel Willie Jones was stopped only a very few days after it began. Starbird asked his Deputies and the Laboratories their opinions on the wisdom of trying to move to Christmas Island under the continued constraint of an April 1 readiness date.

Goeckermann answered for Livermore on February 12:

> In the original planning for Operation Dominic, we were instructed to retain the capability to move ashore at Christmas Island. Therefore, our plan for Christmas Island will closely resemble the Hornet installation. Our site arrangement will probably consist of a control point trailer park located near Able Site, two camera stations located along the south shore, and a rocket launcher pad in the vicinity of Able Site. This basic arrangement has the concurrence of the other technical agencies.

He pointed out that LRL planned to have an advance party of eight arrive at Christmas on or about March 1, but other personnel would not arrive until after trailers arrived. He recommended that all equipment on the ships at the moment be taken to Hawaii on those ships, moved into the dry well of an LSD for transport to Christmas Island, where it would be taken ashore in LCUs. Airlift to Christmas would require considerable modification of the trailers. He recommended against sea transfer of the Hornet trailers using ship's tackle. But overall, he concluded that if we moved immediately the April 1 date could be met. Goeckermann also made the point that a move to Christmas would alleviate the bomb tracking problem, permitting smaller camera fields of view, and, hence, better resolution and improved data. The background problems would probably be less, but the C-130s would have to be based at Christmas.

The other organizations answered in similar vein, and the decision was made to move to Christmas Island on February 15, 1962. The Hornet's Captain was furious!

Move to Christmas Island

Starbird immediately left for Christmas Island, taking along an initial party of Laboratory representatives, some of the Deputies, and an initial crew of H&N and AEC people, and just as he left Washington, he grabbed Colonel Phil Hooper. The initial party promptly completed the layout of most of the technical facilities, the assignment of space in the British facilities, etc. Colonel Hooper was informed that he was the U.S. Island Commander and told to stay there, very much to his surprise.

The scientific and support equipment was removed from the aircraft carrier and loaded aboard a U.S. Navy LSD for shipment to Christmas. Other equipment was packaged and shipped via MATS with great cooperation from CINCPAC, PACAF, etc. There were 367 H&N personnel, 74 user personnel, and 152 military personnel (for a total of 593 Americans) on Christmas Island by February 28. While the Americans promptly took over the control tower and airfield operations, the British, throughout the entire operation, met every plane and briefed incoming people on the hazards of the island.

By the end of March there were approximately 1,500 people on the island. Both the Navy and the AEC moved in communications equipment, the first communication van arriving February 20. However, communications were bad for some time; in fact, they were not satisfactory until after April 22. The mess hall and boiler house were in operating condition within seven days after work started, but the mess hall was never satisfactory during the entire operation: the difficulty was the initial attempt to use British equipment. The barracks were quickly rehabilitated and the water wells and British power plants were started up. The Joint Operations Center (JOC) was rehabilitated and the Air Task Group, TG 8.4, began to move in. (A map of Christmas Island showing locations of some of the facilities is in Figure 13.) Since the British power was 50-cycle and there was not very much of it, American generators were promptly brought in and an extra power system was established near the JOC.

By April 21 new target positions had been picked by joint agreement between the technical Task Units; these varied from 10 to 20 miles from the main concentration of experimental gear which was called A or Able site. A survey of depths and currents off the southern part of the island was made so that target mooring could be designed. The target mooring turned out to be a very serious problem, but, fortunately, between the Navy and Bill Van Dorn a method was developed using lighter-than-water rope and three anchors. It worked nicely.

During late February and early March there was appreciable discussion among the Laboratories, AEC Headquarters, and AFTAC concerning possible measurements by the British on the Christmas shots. They wished to make time interval measurements by EM techniques, for which they had very highly developed techniques. After appreciable discussion their participation was agreed to.

By March 3 the Navy Task Group had arranged to moor targets off Christmas Island and by the end of March they had arranged for placement of the first trial target.

On March 2 the President announced the U.S. decision to test (if the Russians did not come to an agreement before we started) in the latter part of April. The President's announcement allowed a little more time for preparations.

However, on March 7, at the JTF-8 scheduling meeting of the Task Units in Denver, the test organization was told that in order to give the President the option of conducting tests before April 23, preparations at Christmas were not to be relaxed. An appreciable flurry was thrown into the system when it was stated that the President might want to start testing in the atmosphere at any moment. A quick review of the situation led to the conclusion that, if necessary, we could fire within a week. However, it was agreed that the first dry run of the ground-based part of the system (diagnostics, etc.) would be on April 1, that there would be dummy drops between April 1 and 10, drops of high explosives between April 10 and 20, and we would then be ready for the first live round on April 23.

By March 7 Colonel Hooper reported the status of Christmas Island to CJTF-8 as follows:

1. Communications: Conditions appear to be improving, but most frequencies continue to present unsatisfactory reception and transmission. Local equipment deficiencies which have been identified have been corrected.

2. Transportation: Vehicle transportation is bad. Personnel vehicles now being received are in as bad or worse condition than original shipment which you saw here. This has been a psychological blow to all here; however, the shock is about over. A small bus and five pickup trucks have arrived from Hickam. These will be available for customers tomorrow.

3. Main Camp: The northeast, east-central, and central areas are completely ready for occupancy. The west area is about 65 percent ready for occupancy and about 40 percent occupied. Rehabilitation continues. We are placing eight men in six-man rooms. Present water supply will not support more than 1,300 men. Previous

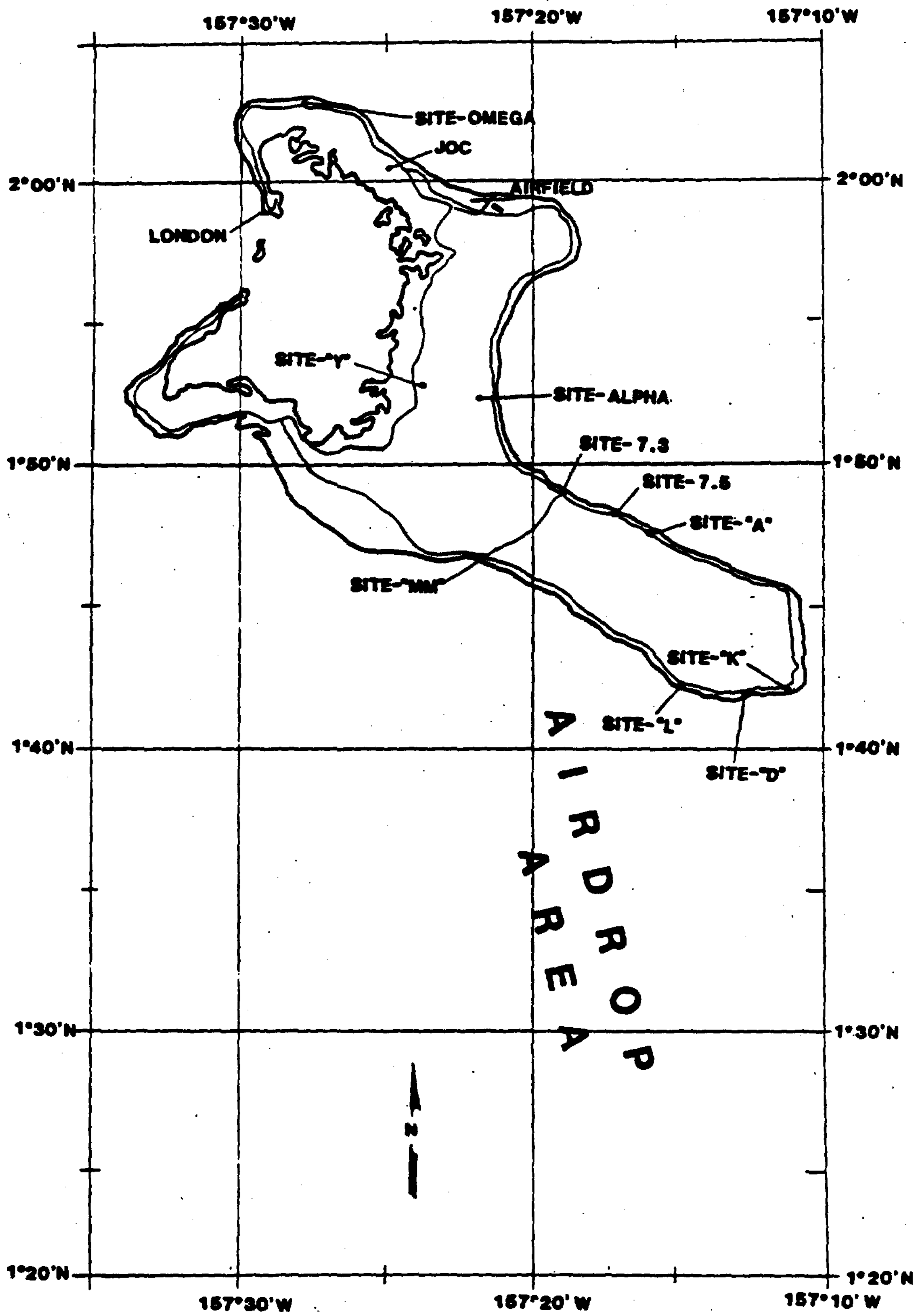

Figure 13.
Map of Christmas Island showing principal facility locations.

U.K. estimates are not proving out. Five distillation units have just arrived on barge. Present population is 1,239.

4. Site "A" Camp: Construction of 250-man camp has been initiated, with site preparations 40 percent complete. Army Port Company is assisting by erecting a temporary camp nearby. This camp will have Port Company kitchen and will permit camp occupancy when trailers arrive.

5. Port Area: To date, the following ships have been off-loaded: Jerome County, Harris County, Monticello, Arikara, three barges, Kabildo, and Snohomish County. The Quapaw, with three barges, has just arrived in harbor with stills and fuels.

6. Airfield construction: Site preparation is in progress. Material and equipment for airfield rehabilitation are due to leave Honolulu on March 7. Field engineering is in progress and preliminary drawings are 30 percent complete.

7. Scientific construction: Site "A"--trailer site is graded and stabilised and material spread ready for fine grading. Trailer site 1,000 feet from main site completely graded. Preliminary drawings and field engineering are in progress. Site MM--access road in and engineering in progress. Site D--bunkers in and shaped. Trailer site is graded and stabilisation material hauled in. Site YY--trailer site cleared, no engineering other than site stakeout.

8. JOC Area: Area has been cleared. In process of restoring air conditioning unit . . .

(Ed. Note: No number 9 included in message.)

10. Fuel Farm: U.S. Marine's units are well along on fuel farms as planned. Two 300,000-gallon units are now going in near Boy Scout-Port area. Two 6,000-gallon units are being installed near U.K. farm at airport. We now have a JPK4 capability. Marines working 24 hours a day and are good . . .

(Ed. Note: No number 11 included in message.)

12. U.K. relations continue as excellent. Accounting systems have been discussed with Mr. Pitman, who is returning London with recommendations for simple arrangements.

13. Colonel Fackler of 8.4 has been most helpful in every way.

On that same day, the scientific trailers arrived. By March 15, 1962, a target raft mooring system had been agreed upon between Scripps and 8.3 and by March 26 the sea bottom survey on the south end of Christmas Island was complete. Thus, by early April the first target raft was in place. The raft was equipped with radar reflectors and beacon lights and also served as the anchor for a small balloon flown at about 1,200 feet to assist ground radar systems. (Some of the rafts survived some of the smaller shots. A picture of a raft in place is shown in Figure 14 and a raft after being exposed to a detonation in Figure 15.)

During March a pipeline was run from the deep sea mooring to the airfield to allow aircraft fuel transport. When the pipeline was first operated hardly anything came out of the far end except land crabs, but it was soon in satisfactory operation.

There were several requests for planned detonation altitudes, many of them incompatible. In order to obtain further calibrations on their long-range seismic and electromagnetic detection techniques, AFTAC requested that the height of burst of some 10 shots be varied between 1,200 feet and 12,000 feet. However, other criteria were more compelling. Because of the cloud layers and the operating height of the C-130s, the Laboratories wanted the burst at an altitude which would guarantee a clear

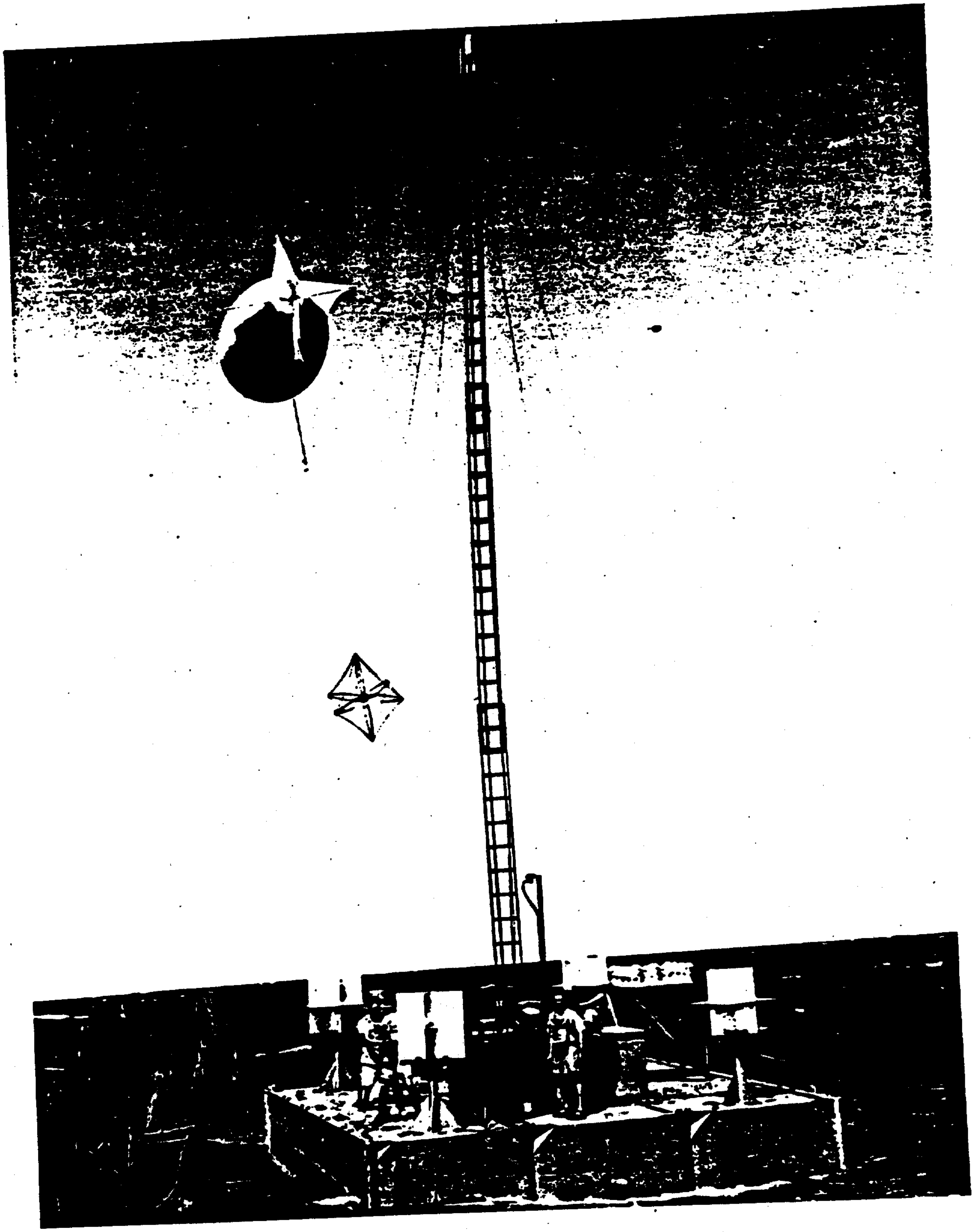

Figure 14.
A Christmas Island airdrop target raft, before detonation: the balloon was called a kytoon.

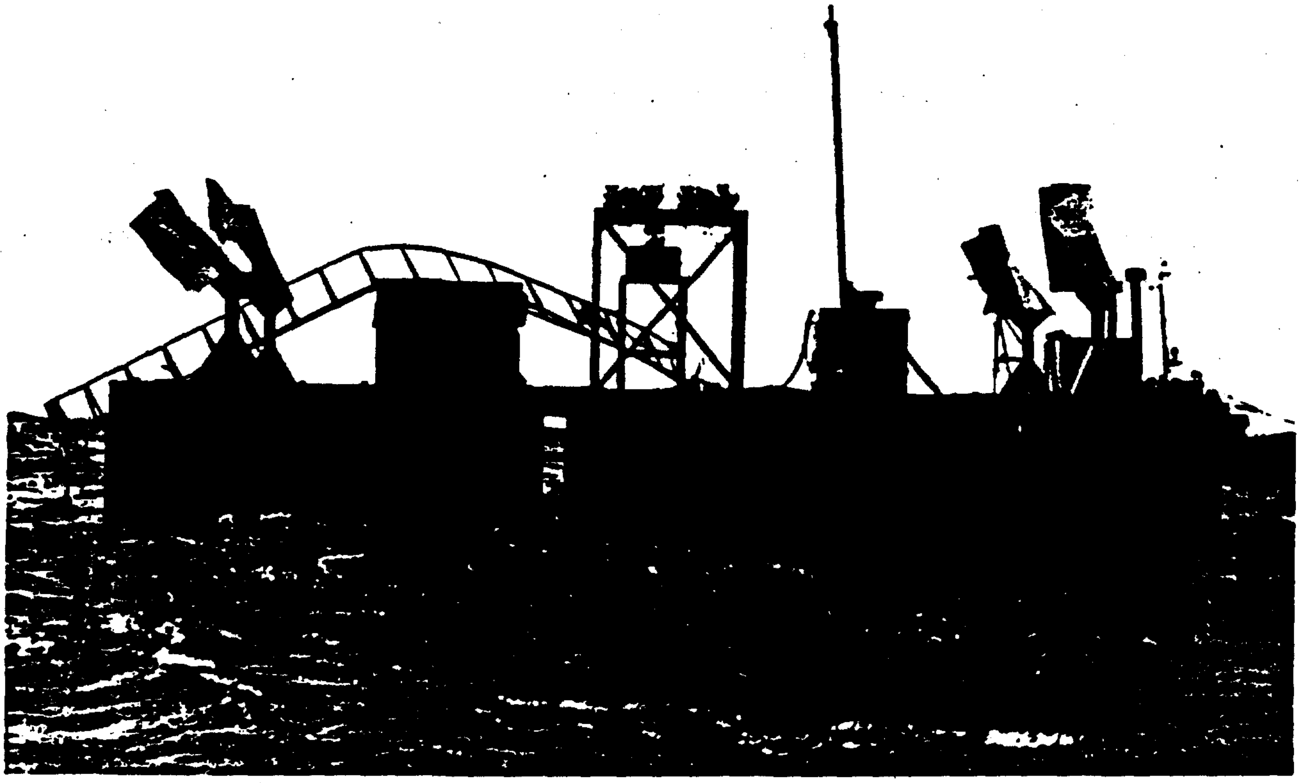

Figure 15.
A Christmas Island target raft after detonation.

line of sight from the ground at A site. The British were, however, concerned with the fallout question and suggested to Ogle use of the following rule:

$$\text{Height of burst} = 270(W)^{0.4} \text{ feet.}$$

On April 5 Jane Hall sent Ogle a message about the planned heights of burst stating:

> We are concerned about rumors we hear that the heights of burst may be chosen without due regard to the primary purpose of the test, namely, to measure the total yield.

Ogle answered on the 7th, giving the planned height of burst for the LASL shots (heights from 2,300 to 5,700 feet), and commenting, "Numbers may have to be changed slightly during operation because of joint fallout consideration between ourselves and the British; however, we will be careful to protect purposes of tests and get data. Don't get excited, I still love LASL best."

By mid-March the B-52 crews had had a number of practice flights from Kirtland AFB, and TG 8.4 was able to report that the crews were up to snuff.

By March 26 Colonel Hooper reported a total population of 1,816. Other items in his status report follow:

Telephones installed to date: 51 in JOC, 56 in Main Camp, 9 in Airport, 2 in Site A, 1 in Site Y. The 70-line exchange will be in operation in A Site by March 25. . . . U.S. mattresses and pillows distributed today in Main Camp. . . . Dining facilities have improved in overall efficiency and appearance. Midnight meals as well as odd hours' servings for air crews and work groups are provided. . . . Camp Store started in March 19 with limited items. Supplies expected by ship next Monday and ready for sale March 29. . . . Average water consumption has dropped the last three days; therefore, more water is available at this time. Distillation units should have pilot run at Main Camp Sunday. . . . Work at the scientific sites has progressed at a faster rate than other projects. Permanent power switchover for next Monday. Site Able permanent camp facilities now available for 200 trailers. Continuing initial shakedown. Satisfactory progress at other sites. Target positions are being checked out by Sandia radar. . . . Air conditioning not yet installed in weather central; therefore, all the electronic equipment cannot operate. Plan to complete by March 28. . . . Airfield: This is the most critical item at the present. Construction equipment difficulties have been encountered. H&N are airlifting additional equipment to include a motor patrol to meet taxiway and parking requirements. . . . Have taken 100,000 gallons aviation gas from tanker to Marine tanks. . . . Fifteen target rafts now moored inner harbor. . . . Medical: Have been informed that a 25th Division medical officer is available. Have requested that he be sent here. . . . Dr. Lee Aamodt's presence has been most beneficial. . . . I do not yet see a solution for latrines at airport and JOC. Shaw is working on this with H&N.

On March 22 Starbird felt the Task Force staff on Christmas was ready to make detailed operational plans, and he notified everyone that Lee Aamodt would be Acting Scientific Director at Christmas until the arrival of Ogle. On that same day Ogle sent Aamodt some suggestions on the height of burst for the first shot; tolerances on the target position; suggested operational communications, including TV reproduction of the Sandia plotting board at the JOC; muster and security-sweep items; etc.

At the last minute several other experiments were added. In late March AFSWC obtained permission to use their own B-57-B aircraft to determine the thermal effects of low-altitude nuclear detonations on aircraft. In addition, Guthals arranged for debris cloud pictures to be taken from the sampling B-57s until 2 hours after detonation. Finally, DASA requested approval to do the eyeburn experiments using monkeys and rabbits, which had been suggested for Operation Everready.

[redacted]

Withheld Under 5 U.S.C. 552(b)(1) 1.3(a)(5) EXEMPTION 1

The problems of protecting the natives on the island occupied an appreciable amount of time in late March, the initial decision being to build a fence 15 feet high behind which they could be placed so they could not see the initial detonation, thereby preventing eyeburn. Food and entertainment were to be furnished at shot time. In case of fallout, it was suggested that the natives be moved to their stone church at London.

Late in March the British representative, Air Vice Marshal McKinley, was convinced by Starbird to agree to the JTF-8 proposal that the danger area include Washington and Fanning Islands, for which there would be specific protective measures.

On April 3 Starbird and Ogle briefed the Governor of Hawaii on the forthcoming operation, assuring him that there was no problem to that territory. Subsequently they went to Christmas Island and established the Task Force Headquarters there, effective on April 4. Late in March TG 8.4 had moved to Hickam and the B-52 bombers moved to Barbers Point NAS. Practice drops of mock drop vehicles (shapes) were made on April 6, 7, and 8, 1962, and everything operated properly. However, on the 10th the first dry run occurred using all the appropriate electronics, beacons, fusing, etc., (DRM No. 1), leading Ogle to comment in his notebook:

> The dry run on the 10th taught us a lot. The system for getting information from A Site to the Air Operations Center (AOC) was bad, to the Joint Operations Center (JOC) was impossible. The bomber and device beacons could not be picked up, part of the telemetering would not work, the bomber made his first run on a ship, etc. We aborted the first live run (9 a.m.) at minus 11 minutes, let him go on the second to minus 20 seconds, and then aborted and sent him home.

On April 13 the second practice run operated properly. The next dry run, on April 16, was moderately successful except that Sandia lost tracking and had to go to a pre-set position for the cameras, and that operated properly. Communications to the control room were still bad. Dry run No. 4 on April 19 was aborted because of weather (the practices were being done realistically), but it was completed successfully on April 21.

On April 19 Starbird received a message from Luedecke warning him that the Presidential announcement of the U.S. intention to return to testing was expected on the 24th of April: by the 23rd, with only a one-day notice, the system was ready. During the afternoon of April 24 Starbird received a message from Betts transmitting Presidential authority to begin testing.

To summarize the situation at Christmas Island at that point: Bombs were to be dropped on a target (see Figure 15) which was roughly 10 miles from a manned experimental station, the range depending upon the expected yield of the bomb. The station was instrumented jointly by LRL, LASL, EG&G, and Sandia to perform optical and electromagnetic time interval measurements, and to take fireball pictures. Fireball pictures were also taken from a second station. Electromagnetic time interval measurements were made from several points on the island. The same types of measurements were made from the C-130s based at Christmas Island. The Sandia radar continuously tracked the drop aircraft in its orbits and presented that information at A Site, the Headquarters for the technical organizations. Information on aircraft positions could be sent to the JOC either by solid wire from A Site or by radio from the RC-121 control aircraft. The air array positioned itself on the target raft. Sampling aircraft operated out of Christmas and samples were to be returned directly to the Mainland by special C-135 airlift. The aircraft inventory at Christmas Island on April 22 is shown in Table XXXVII.

Other parts of the system which were based in Hawaii consisted of B-52s operating from Barbers Point and the LASL optical KC-135 operating out of Hickam. The KC-135 had been obtained for high-altitude operations, but also carried out long-range optical detection experiments on a number of the Christmas Island tests. Lee Hollingsworth of Sandia, who had been put in charge of all weapons, was also based at Barbers Point, where a weapon assembly facility had been established. Livermore, LASL, and Sandia weapons experts were also based at Barbers Point.

The radiological safety organization set up under Gordon Jacks was based in Hawaii, but it was responsible for those activities at all sites.

Just before the tests started General Samuel had found it necessary to establish another Task Unit at Christmas, headed by Colonel Paul Fackler. Dan Rex had taken on the responsibility of putting together a weather and fallout prediction system on Christmas Island, and various members of the Ad Hoc Safety Panel (Orin Stopinski, Vay Shelton, etc.) were always present to assist in judgments about hazards on the shots. As agreed upon with the British, the final Safety Panel included Air Vice Marshal McKinley, whatever Deputy Commanders were present (all were there for the first shot), and the Task Unit Commander representing the Laboratory whose shot was being fired. The Safety Panel was chaired by the Scientific Deputy or, in his absence, his designated alternate (in general, either Aamodt or Goeckermann). The Laboratory Task

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Unit Commander had final say on behalf of the sponsoring Laboratory, that is, he could always stop the shot, but he could not turn it on without agreement from the Safety Panel and the Task Force Commander.

TABLE XXXVII
AIRCRAFT ON CHRISTMAS ISLAND
April 22, 1962

Type	Number	Unit of Assignment
WB-50	5	55th WRS, McClellan AFB, California
B-57 B/C	11	1211th Test Sq., Kirtland AFB, New Mexico
B-57 D	6	1211th Test Sq., Kirtland AFB, New Mexico
B-57 B	2	Aeronautical Systems Division (USAF), Wright-Patterson AFB, Ohio
B-57 D	1	Hughes Aircraft
RC-121	2	52nd Airborne Early Warning & Control Wing, McClellan AFB, California
C-130	2	Stewart AFB, Tennessee
P2V	14	Navy
C-54	2	APCS
SC-54	2	Air Rescue Service
H-21	6	Stead AFB, Nevada
C-118	1	General Starbird
L-19	1	General Starbird

NOTE: B-52s and C-135s (sampler return) at NASBP.

The Jarvis, Baker, Howland Connection

[redacted] Within the Laboratory the choice quickly became either Jarvis Island or the Baker-Howland group. A search of the World War II records showed that it would be difficult to operate landing ships at Baker and Howland, which had been used as staging islands for aircraft going into the Pacific theater. The island had to be fairly large and have some moderately flat area for the very large experimental array planned.

As a result of the President's unhappiness about the number of shots [redacted] there was an attempt to combine the Livermore (Sioux) vulnerability and effects experiments with the LASL shot. LASL also introduced into the experiment some vulnerability measurements. However, the basic experimental arrangement would consist of a multiplicity of long pipes fanning out in all directions from the device, each pipe fitted with appropriate neutron detection systems on the end (Phonex). LASL also planned to make close-in electromagnetic effects measurements on this shot.

On November 30 LASL asked the Task Force and the AEC to begin looking for an island, and suggested Jarvis as a first try. Colonel McMillan of the DMA test office met with representatives of the State and Interior Departments on December 11 to discuss the possible use of Jarvis, Baker, or Howland. Since the islands belonged to

Withheld Under 5 U.S.C. 552 (b)(1) NSC, EXEMPTION 1
NSA 552 (b)(3) DOE EXEMPTION 3

Withheld Under 5 U.S.C. 552(b)(1)
Also Exemption 1

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the U.S. and were uninhabited, State had no concerns from a political point of view, but were concerned with the fallout hazard. The birds on Jarvis were mentioned, but without concern. The conclusion of the meeting was that DMA should send letters to both the State and Interior Departments describing the proposed uses of the islands, outlining the safety aspects, and requesting approval. The State and Interior Department representatives felt an affirmative answer would be forthcoming. On December 22 H&N began to estimate costs for the test preparation work on the island. After a bit more study DMA recommended the use of Baker, if possible, rather than Jarvis because of Interior Department information that there were about a million birds inhabiting Jarvis Island. On the other hand, Ogle and Starbird preferred Jarvis because it was closer to Christmas and sampling for a test done there would be possible using aircraft operating from Christmas. On December 27 Bradbury formally asked Betts to arrange for the Laboratory's use of Jarvis and to notify the Lab of the island's availability by the first of the year. On that same day Ogle and Starbird agreed on a danger area around the island, 250 by 400 miles on a side, with most of the area downwind.

The LASL operational concept as of early January 1962 was to occupy a camp on the island on the 1st of April, spend the next six weeks preparing scientific stations, evacuate the island on May 14, and fire on the 15th. Thus, H&N would have to deploy to the island, build a camp, and get heavy equipment there by April 1, only three months hence. A 10-ton crane, bulldozers, and other vehicles would be necessary, and somewhat more than 50 technical people would be on the island then. LASL requested a ship to hold some of their nine trailers, and, in addition, adequate ship-to-shore transportation. Since the fireball yield was desired, fireball camera stations would be built on the same island as far away as possible from ground zero. That requirement made Jarvis look a little better than Baker. If Baker were picked, sampling might be done by aircraft based at Canton Island, and if Jarvis were picked, sampling might be done by aircraft from Christmas, if we had use of it. If not, perhaps samples could be obtained by A4D aircraft operating from a Navy carrier, and Admiral Mustin was queried on that point. LASL requested that an LSD-sized channel be opened into Jarvis, and that, while waiting, any maps available, overhead photography, etc., be obtained. JTF-8 promptly asked CINCPAC to arrange for overhead photography of Jarvis and Baker Island, the results to be provided as soon as possible, and also requested that they plan an inspection trip to all three islands to begin on January 22.

On January 19 Secretary Udall informed Seaborg that Jarvis would be acceptable as a site. Although a large number of birds would be destroyed, there was no danger of extinction of any bird species. Udall concluded that the military necessities for the shots overrode the substantial wildlife losses.

The late January survey of Jarvis and Baker showed that appreciable blasting would be necessary to clear a boat channel into Jarvis, that the seas were very rough, and that nothing larger than an LCU would be feasible for putting equipment on Jarvis. Baker would take about the same effort, but the World War II airstrip could be made operational in about a week by ten men with some equipment. It was estimated that the number of birds on Baker was about one-tenth the number on Jarvis. By the end of January Starbird had indicated his unhappiness at using Jarvis, because of the birds, and had asked Farley of the State Department to investigate the possibility of using Canton Island for sampler aircraft operations and logistics backup. If that were feasible Baker might be a better choice than Jarvis, assuming we did not get authority to use Christmas Island. Since sovereignty over Canton was also contested between the Americans and the British, Farley thought it would be just about as difficult to arrange use of that island as it would be to use Christmas. By the end of January a complete experimental plan had been transmitted to those involved, the logistics requirements were known, H&N was designing the construction required, and LASL was building the detector systems and other equipment needed for whichever site was selected.

On January 31 George W. Ball, Acting Secretary of State, stated there was no objection to the use of any of the three islands, provided precautions were taken to avoid hazards and fallout, but he did feel that it would be advisable to take any reasonable steps to minimize destruction of the birds. Upon their return, the survey team strongly recommended the use of Baker Island since, with a minimum amount of work, the beach at Baker could be usable for landing craft, not so many birds would be killed, and the airstrip could quickly be made operational. The requirement for fireball cameras was canceled after Livermore agreed that flying both C-130s on the Baker Island shot would provide sufficient fireball photographic data.

On February 6, 1962, Bradbury offered to forego the Baker Island shot in partial exchange for approval of the deep space shot, Urraca. Betts and Brown took him up on it, and the Baker shot was canceled (as was Urraca, later). The vulnerability and EM experiments were instead transferred to the NTS Smallboy shot.

Betts put it slight differently to the Commission. On February 7, 1962, the Commission Secretary, W. B. McCool, recorded:

> **General Betts stated that the Los Alamos Scientific Laboratory had recently submitted a proposal for a 1,000- to 2,000-kilometer atmospheric test to replace a test previously proposed for Baker Island. The Chairman said that he was in accord with LASL's recommendation, although he would like the Department of Defense to concur in it. The Commission approved, after coordination with the DOD, planning for the 1,000- to 2,000-kilometer experiment.**

The Navy had already gone ahead on procurement of the ship Monticello for support of the Baker Island operation, and the Air Force had indicated a need for 43 officers and 90 airmen at Canton in support of that effort. All of this was canceled on February 8, 1962.

Swordfish (ASROC Effects Test)

[redacted] Furthermore, it was initially planned for execution in the Atlantic, but now a Task Force had been formed. Thus, two changes had to be made by the Navy. One was to put together an entire effects program that would fit with the firing of the ASROC device, and the second was to decide what DOD organization would be responsible for the operational aspects. It did not take long to decide the relevant responsibility question. To Booth it was obvious that since ASROC had been redesignated as an effects test, it should now, by military rules, come under DASA, who were, in principle, in charge of effects experiments. Thus, Booth suggested to the Joint Chiefs of Staff on December 12, 1961, that ASROC responsibility should be assigned to JTF-8 under Chief, DASA. There were obvious difficulties because ASROC had been planned for the Atlantic and Starbird had made it clear that he did not wish to operate in two oceans. Starbird also told Booth that if the operation were conducted under JTF-8 it would not be under Field Command, DASA, but would be directly under JTF-8. The ASROC shot was assigned to JTF-8 on January 12, 1962, for incorporation into the Dominic series.

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Since the test was now an effects test the Navy had to come up with an effects experiment test plan, which they did in just a little over two weeks, with appreciable assistance from the David Taylor Model Basin organization. By February 2, 1962, the Navy outlined the objectives of the experiment as follows:

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1.3 (a)(1)
DOD, Exemption 1

On that same day DASA released funds to the organizations responsible for the Swordfish projects and started planning the measurements. The proposed date for the shot was May 1. On February 21 Mustin assumed operational technical cognizance of Swordfish and on March 3 he announced the formation of Task Unit 8.3.4, specifically assigning that group the mission of planning and coordinating the Swordfish test.

Since the operation would need facilities of the same type used for other portions of Dominic, the Task Force Headquarters initially planned that Swordfish be done near Christmas Island, somewhere in the danger area to be established for the airdrop operation, and they so announced on February 20. The President, on March 2, announced that testing would resume and on the same day Task Force 8 publicly announced its formation and mission, stating that the detonations would be carried out in the Johnston and Christmas Island danger areas. Three days later, Roger Revelle, who, as Science Advisor to the Secretary of the Interior, had been involved in many of the earlier Eniwetok/Bikini operations, wrote to the AEC expressing his concern about the test of the ASROC in the Christmas Island area and suggesting instead the Wigwam area off the coast of California. As background for the suggestion he noted that the Wigwam area had been studied extensively in the past from an oceanographic and biological standpoint with the result that "virtually no marine life of economic importance would be affected in the Wigwam area, and these conclusions were borne out by the observed distribution of radioactivity after the tests." (The Wigwam event occurred in 1955.) He also wrote: "On the other hand, the Christmas Island region is close to one of the most fertile areas in the ocean and is extensively used by Japanese fishermen."

Starbird discussed Revelle's suggestion with Luedecke on March 14, noting that he had planned so far to conduct the exercise in the Christmas area in order to utilize resources that would be there for other purposes. With respect to moving the event, he said:

> It would require substantial added resources to conduct this experiment in the alternate Wigwam area. We shall immediately investigate what is needed in this regard. In addition, it is probable that use of the alternate area would require the declaration of a danger area not now contemplated, although no such danger area was declared for the Wigwam event in 1955.

Since the selection of the nuclear weapon test sites had been assigned to the AEC, General Starbird raised the question with the Commission during his briefing on March 28, outlining the whole program. Starbird noted that while he was aware that the Wigwam area was known as a "fish desert," he had planned to conduct the shot in the Christmas Island danger area for operational reasons. He had read the preliminary comments of the Division of Biology and Medicine (AEC), which concluded that a very few commercial species of fish would have a measurable but not hazardous level of radioactivity, and probably the number of fish which could be caught and have a measurable degree of radioactivity would be between 7 and 70. However, he noted that both Scripps Institute of Oceanography and the Wood's Hole Oceanographic Institute recommended that the shot be moved to the San Diego area, and that Secretary of the Interior Udall had also asked the AEC to reconsider the planned location of the test. He went on to say that he would continue to recommend that the test remain in the Christmas Island danger area, but if it was moved to the Wigwam area, he would ask that the Navy conduct it. Mustin noted that if the tests were conducted in the Wigwam area, it would be necessary to acquire the services of an additional LSD, an additional tug, approximately six P2V aircraft, and three additional aircraft for weather reconnaissance information. He could not comment on the availability of the additional resources at the required time. The planned shot date was only six weeks away (May 18 to May 23).

The Commission discussed the subject again on March 30 with Roger Revelle, Spurgeon Keeny, Gerry Johnson, Admiral Mustin, and Dan Rex (the Task Force weather officer) present. Task Force representatives continued to favor the Christmas Island area. Mustin revised his estimated added requirements to approximately ten aircraft, three ships, four small Naval craft, and 800 men, if the test were conducted in the Wigwam area. Dan Rex presented data to show that the weather was somewhat more favorable in the Christmas Island area and Mustin estimated that it would take approximately five days to conduct the test in the Christmas area, but ten or eleven days in the Wigwam area.

The representative of the AEC Division of Biology and Medicine, Dr. Dunham, opined that it was virtually certain that someone would discover tuna or another commercial fish which would have experienced a measurable but not dangerous increase in radioactivity as a result of the Swordfish test, if conducted in the Christmas area. After discussion among Revelle, Dunham, and Mustin, it appeared clear that there was no actual hazard, but there could be no guarantee that the Japanese, who fished the area fairly heavily, would not catch some nonhazardous but radioactive fish. The result was that in view of Revelle's position, and as public evidence of the AEC's concern with safety, the Commission approved a relocation of the Swordfish event to the Wigwam area.

General Starbird raised the question of a Wigwam exclusion area with the Commission on April 12, stating that the area should be some 70 by 100 miles, but also noting that the absence of commercial fishing vessels with only a small amount of merchant shipping in the Wigwam area supported his recommendation against establishing a danger zone. Gerry Johnson concurred for the MLC. The Commission agreed it would not be desirable to establish an additional danger area at the Wigwam site.

Thus, by mid-April the Navy laboratories (NOL, NEL, NRDL, etc.) were prepared to make appropriate measurements in the water, in the air, and on the ships. EG&G furnished the common timing signals and the AEC took on the job of monitoring the radioactive pool and determining the marine life effects. A towed test array was designed consisting of a ship and a number of coracles from which hung instrumentation. Bill Murray of the David Taylor Model Basin was the Scientific Director for that shot and Capt. Ben Petrie was the Task Unit Commander.

Thus, by April 24, when the President announced the resumption of testing, the Swordfish system had been designed, most of it put together, and the shot point had been chosen. The system was being assembled in San Diego and was preparing to rehearse.

The Polaris System Test

After President Kennedy's March 2 announcement on test resumption the JCS promptly requested that the previously considered Polaris system test be included in the series, and by March 7 it had been approved. The appropriate command and control systems had already been developed before the November 29, 1961, NSC subcommittee meeting, which had decided to delete the test nicknamed Frigate Bird. The appropriate missile and warhead destruct systems had also been designed and were being built in the interim.

The reinsertion of this shot at such a late date caused an appreciable flurry in the Task Force. Starbird and Ogle had no time for detailed study of the fusing and firing systems because they were deeply involved in preparations for the Christmas Island and Johnston Island operations. Scheduling was an immediate problem, planning still being constrained by the Presidential directive to make the operation as short as possible, once started. Starbird felt very strongly that the high-altitude portion of the operation would demand great attention and that, therefore, Frigate Bird could not be fired during June, or even in the latter part of May. (The three high-altitude shots were scheduled for June 1, June 5, and June 30, roughly.) The Polaris boat Ethan Allen was in the Atlantic at the time the decision was made, and no one wanted to consider establishing another danger area. Furthermore, it would be better to have the shot before Tiger Fish (the first Thor launch from Johnston Island), which was scheduled for May 1, but the Ethan Allen could not get to the Pacific that soon. Therefore, Starbird insisted that the Navy move as fast as possible in order to fire during the first week of May.

A second problem was the determination of the launch and burst points. The initial suggestion was to fire into the Christmas danger area from a position near Johnston. This had several operational advantages, among which were that the Task Force Commander would be able to spend some time satisfying himself concerning the launch safety conditions, and observations could be made using equipment at Christmas Island. However, it had two fairly serious disadvantages over the other possibility, which was to launch toward Christmas from a point roughly due east of Christmas. The first disadvantage was scheduling. The Ethan Allen had to transit the Panama Canal, and the travel time from there to Johnston Island was appreciably longer than to the position east of Christmas. Secondly, while there clearly was a range safety system of sorts and a missile destruct system, there was not time for Ogle and his safety advisors to go into the details. Therefore, it seemed wiser to launch the missile from a point where its range would preclude reaching inhabited land, no matter what

Withheld Under 5 U.S.C. 552 (b)(3), DOE, Exemption 3

its trajectory. Thus, a launch point was picked more than 2,000 miles east of Christmas Island, with the burst point somewhat to the northeast of Christmas. To preclude serious eyeburn [redacted] the burst point was to be far enough from Washington and Fanning and other inhabited islands that people on those islands could not see it. It is interesting, in retrospect, that we were willing to launch from a point outside the danger area, but apparently the point did not arise then.

The Navy was anxious to specify the burst point and the yield of the device since, after all, the point of a systems test is to prove that everything operates correctly. The initial plan did include a plan to determine the burst position and yield by using two submarines near the burst area equipped with cameras and bhangmeters operating through a periscope. In addition, a plan was made to use the airborne diagnostic equipment devoted to the Christmas Island operation. Thus, in the last few weeks before the first nuclear test there was quite a flurry of activity on Christmas Island in an attempt to set up such a capability. Both the Navy and LRL also wanted radiochemical samples for yield determination, if possible, and TG 8.4 studied that possibility during those last few weeks.

A further serious problem was communications. Starbird felt that the test should be under the operational control of Admiral Mustin, the Navy Deputy, and, furthermore, he could not himself afford the time necessary for sea transportation to a ship 2,000 miles away. However, Starbird took his responsibilities strongly to heart, and therefore felt that he had to have good communications with Mustin. Thus, in still another way, the last few weeks before the beginning of the operation involved a great deal of effort trying to establish effective and reliable communications. Unfortunately, proper communications checks could not be made until the ships were near their designated area, which was to be some time after the beginning of Dominic. A further need for good communications was to notify the diagnostic aircraft and submarines of burst time, and doing this required prompt notice of launch time from the Ethan Allen.

Once permission was given, the Navy moved rapidly. The four test missiles were modified by the Navy Weapons Annex in Charleston under the technical direction of the Navy Special Projects Office in time to allow sailing of the Ethan Allen on April 19. The missiles were provided with a destruct system and a beacon for tracking by the missile flight safety ship, the Norton Sound. Tests to ensure compatibility of the destruct systems on the Norton Sound and on the missiles were made using equipment flown back and forth across the country several times. Appropriate parts of the flight safety system were installed on the Norton Sound at the Pacific Missile Range yards, and she sailed on April 27, eight weeks after preparation for the tests was ordered. Other equipment on the Norton Sound included gear for special underwater and radio communications needed to communicate with the submarine in the submerged condition and with the Task Force Commander at Christmas Island.

Thus, when the President finally ordered the resumption of testing the Frigate Bird array was already at sea.

The Atlas System Test

The JCS and the Air Force also obtained approval during the week of March 2, 1962, to reinstate the previously proposed test of an Atlas missile system. The shot would be fired from Vandenberg AFB in California to a target area somewhat north of Johnston Island, but in the Johnston Island danger area.

Starbird, whose arm had been twisted to induce him even to continue taking responsibility for the Polaris shot, could see no way to take operational responsibility for the Atlas effort. The attitude was not displeasing to the Air Force, so the arrangement was quickly made that the Air Force would be responsible for the launch and for range safety near the California coast, and the Task Force would be responsible for safety on the far end and for what diagnostics would be accomplished.

It is noteworthy that the AEC laboratories were not particularly happy about these added tests. From their points of view, it was the same old thing that had happened many times before. The military proposes a systems test; tells the President that they can do it in zero time because, after all, it is an operational system; and then when approval comes, they need help. The point of a systems test is, of course, to see whether the system will work, including the final explosion. The capability to determine yield and time interval, as minimum diagnostics, lay in the AEC Laboratory Task Units. The logistics effort for accomplishing these purposes, while in general military, had been assigned to the Laboratories and the Laboratories' hands were completely full with their own jobs. Thus, when Ogle talked to the Task Unit Commanders responsible for the AEC diagnostic system, there was no great enthusiasm for helping. The obvious technique, of course, would be to use the C-130s and the array control C-121 from Christmas, positioning an air array at a safe distance from the intended burst point. If fireball pictures were needed, it would be a difficult job to arrange the proper camera pointing without tracking beacons, etc. Use of bhangmeters and electromagnetic time interval measurements would be simpler. The result was that while concepts were bandied about, no detailed diagnostic plan for the Atlas test had been developed by the Task Force organization prior to the resumption of testing.

It took the Air Force a little while to get the Atlas test program started. In mid-March 1962, apparently because of possible safety problems and because Johnston Island was too close to Vandenberg to allow the desired trajectory, they were considering starting tests in mid-1963 in order to have time for reasonable planning. At that same time, they were considering target areas near Wake and Taongi. However, they apparently also realized the nature of the political situation, and, by the end of March, were investigating the possibility of doing the shot as part of the 1962 series. By then there had been enough discussion with the Task Force and the other portions of the system for the Air Force to realize the desirability of a warhead destruct capability during powered flight. The possibility of installing such a destruct system, however, did not appear likely in the time available, and the Air Force Ballistic Systems Division (BSD) continued to fuss with the problem. By mid-April the concept was to use the Johnston Island danger area for the target zone, and AEC approval was being discussed.

The principal concerns were safety related. In order to reduce the probability of an inadvertent nuclear detonation to about 10^{-7}, the Air Force could incorporate an additional prearming system into the warhead. The major problem, however, was the possibility of spreading some of the Mark 49's [redacted] around Vandenberg AFB if the missile were to go awry during launch or the early stages of powered flight. On April 19 the Commission decided that they would wait for a recommendation by the Secretary of Defense before approving the test.

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The Johnston Island Buildup

The decision to use Johnston Island led to an immediate need to visit the island in order to make detailed logistics and construction plans. However, in early December 1961 that was difficult because while we had been told to prepare, and the Task Force had been established and was being staffed, we were also told to keep everything quiet and, specifically, to keep the existence of the Task Force quiet. Consequently, the necessary arrangements were made through AFWSC and CINCPACAF. The survey team left Hawaii for Johnston on December 13 and spent the 14th making a quick survey and preliminary layout. In a few days the party had grown and a special airlift was arranged. In addition to Starbird, Mustin, and Ogle there were other representatives: Pat Ryan, Marty Curran, and John Pollet of H&N; Hittidale and Arthur from Douglas; etc. A quick look at the island revealed many problems. The power system was in poor shape, as were the barracks; water supply was inadequate; etc. Nevertheless, the party quickly laid out the beginnings of an island facility, including tentative positions for hospital facilities and headquarters of the various organizations. In addition, the Task Force missile group and the Douglas representatives learned the difficulties of building a launch pad by March in order to be ready for a Thor certification shot on May 1.

Thus, by December 7 Starbird had sufficient basis to make a preliminary estimate for the JCS of all the forces required, including ships, aircraft, etc. Better estimates were made in a December 11 meeting in Washington:

a. Three LSDs in the forward area by May 1 to serve as launching platforms for instrumented sounding rockets and as instrument receiving ships.

b. Two destroyers in place by February 15 to function as weather stations making upper air observations, and approximately four destroyers or destroyer escorts in May to conduct surface surveillance patrol and act as instrument receiving platforms.

c. Four LSTs/ships in January and February to provide transport between Pearl Harbor and Johnston Island for the buildup-support role of Johnston Island.

d. Sixteen Navy aircraft by March 15 to conduct air surveillance and antisubmarine patrol in the open ocean and Johnston Island areas.

e. Approximately five C-135 aircraft for optical and photographic measurements.

f. RC-121 aircraft by March 1 for use as airborne control aircraft.

g. An additional C-130 for high-altitude diagnostics measurements.

h. Two U2s by May 15 for very high-altitude weather photography.

i. Ten WB-50s for weather reconnaissance.

j. A VC-121 by February 15 as a transport aircraft for the Commander and distinguished personnel.

k. Three C-54s by March 1 for documentary photography.

A number of other aircraft and ships were mentioned to support separate experiments, and even a small boat pool was mentioned.

By the third week in December 1961 the DMA authorized ALOO to direct H&N to begin hiring personnel at a high rate, and the Task Force began negotiations with PACAF to take control of Johnston Island. H&N was to supply the support facilities and the AEC would be reimbursed for non-AEC users. The proposed JTF-8 Headquarters was on Ford Island because of the open sea operation, and work had already started in designing the rehabilitation necessary there. Other moves started in that same week; design for rehabilitation of the barracks on Johnston Island; modification of the MATS freight terminal to take care of the increased load; modifications of both Hickam and Barbers Point to accommodate the increased activity; moves toward getting Douglas started on a program of inspection, checkout, and packaging of the Thor and working with H&N for the design of the necessary construction work on the Thor pad; steps to authorize the proposed test firing of the Thor at Vandenberg; appointment of a military commander to push the Thor effort; authorization for H&N to move onto Johnston Island on January 3; and transport of supplies to Johnston Island by December 26 so H&N would have necessary material to begin work.

Security classification raised its ugly head. The contractor personnel to be moved onto Johnston Island had to have [redacted] security clearance. A cover story had to be prepared to hide the increased H&N activity, both in Hawaii and Johnston Island, and a plan was prepared for dealing with the Hawaiian authorities in case of a leak.

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An almost continuous series of meetings was held in the last two weeks of December and the early part of January among all of the participants to define the operational concept, the construction requirements, the logistic requirements, etc. The question of aircraft was a serious one. Both LRL and LASL wanted to instrument aircraft (preferably C-135s) for optical observations of the high-altitude shots, and DASA and AFSWC wished to do the same thing (and, of course, felt that they had priority on use of the aircraft). A series of discussions among the Laboratories, Field Command, and AFSWC in late December and early January tried to settle this problem. At the same time discussions began with the FAA to establish control of Pacific air routes during shot time to prevent hazard to commercial aircraft. During meetings near the end of the year the following arrangements were formalized:

a. Sandia would deal directly with Douglas and AFSSD on the problems of mating the capsule containing the test device to the Thor and all the electronic problems related to firing, fusing, safety, etc.

b. Colonel E. A. Meyer was assigned as AFSC project officer for the high-altitude program.

c. Douglas would immediately begin work to define Thor trajectories, including the effects of winds on the accuracy of warhead positioning.

d. The Task Force would arrange for range tracking.

e. As a deputy to Ogle, Colonel Rod Ray would be responsible for all weapon carrier missile problems.

f. DASA would review their need for nose pods.

g. Communications needs would be coordinated directly with Colonel W. A. Randall of JTF-8/J-5 or Sam Howell of H&N.

h. Shipping requirements and weapon movements should be coordinated with Captain George Waite of Task Force J-4.

Late in December 1961 DASA Field Command issued a tentative weapons effects program for the DOD portions of the tests, listing the purposes of the program, the information sought, the types of measurements to be made, the tentative project agencies and project officers, and estimated costs. The plan included the two shots, Starfish and Bluegill. Some of the purposes given were as follows:

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DOE, Exemption 3

Four main programs were outlined, each containing several projects. The programs were blast and shock measurements, nuclear radiation and effects, electromagnetic phenomena, and thermal radiation effects.

Among the techniques to be used were fireball photography from the ground and the air; neutron threshold and gamma detectors in the pods; neutron and gamma detectors in small rockets; observation of transient electromagnetic effects on various radar and communication systems; radio transmitters to be carried on small rockets to the appropriate place with respect to the fireball for ground observation of transmission; spectrometers and various flux rate meters and particle collectors on small rockets; resonance scattering measurements; observation of magnetic field changes and motion of the debris by small rockets; observation of cosmic noise attenuation; observation of radar echoes, clutter, and scintillation using equipment mounted on ships; ionospheric observations from a KC-135; satellite observations of trapped electrons, magnetic field fluctuations, fission fragments, synchrotron noise, and x-radiation; scanning spectrometers and black body bolometers; high-speed streak and framing cameras; lower-speed technical photography; total thermal energy versus time; observation of the effects on recoverable pods due to debris and heat using impulse and ablation gauges, calorimeters, accelerometers, etc.; x-ray flux and spectrum measurements from detectors on small rockets and satellites; indenter gauges to measure the total momentum of particles; and others.

Sandia designed a "mylar sail" to sample the debris from the high-altitude shots. It was to be raised to high altitude by a small rocket, deploying a large sheet of mylar at 80 kilometers during ascent. The "sail" would then sample from 100 kilometers to 220 kilometers altitude, at which altitude the sail would be retracted into the nose cone and sealed. Following splashdown of the nose cone, the system would be recovered from the water. The nose cone was built to float and had the appropriate radio aids in it for signaling its location.

During the last week of the year the first increment of Holmes & Narver construction personnel arrived on Johnston Island and the first shipment of 1,000 tons of construction equipment and supplies departed Pearl Harbor by barge en route to Johnston Island. During the first week of January Brigadier General Eugene A. Salet, U.S. Army, arrived in Washington for briefings before assuming his duties as Commander of Johnston Island.

By the second week of January 1962 arrangements had been made for Sandia to use the Barking Sands facility on Kauai for their small rocket program; negotiations were essentially complete for transferring control of Johnston Island to JTF-8; and Field Command DASA was prepared to send a group of people on a tour of the Pacific to arrange sites for the appropriate instrumentation. Some of the sites selected in addition to Johnston Island and Christmas Island were Oahu, Maui, Kauai, Hawaii, French Frigate Shoals, Midway, Wake, Okinawa, Kwajalein, Palmyra, Canton, Fiji, Samoa, Tongatapu, Rarotonga, Adak, Fairbanks, and Palo Alto.

Other important parts of the system had also been defined. Dan Rex had recommended islands to be used for weather observations and necessary use agreements were underway. The safety system was started. At Ogle's suggestion Starbird selected Gordon Jacks to be responsible for the rad-safe organization.

A most important meeting took place on January 9 among the AEC Laboratories and Field Command DASA during which it was decided that LASL would procure [redacted]

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Sandia agreed to attempt a real-time presentation of the radar tracking data they were acquiring for their own purposes, and the Cubic Corporation, under contract to DASA, also agreed to provide additional tracking data (as a backup) from a transponder installed in the Thor.

On that same day, at General Samuel's urging, Systems Command established Task Group 8.4 (provisional) at Kirtland Air Force Base. In the first week of January the AEC assigned the nickname "Dominic" to the possible forthcoming atmospheric operation in the Pacific. The Air Force also changed their support nickname from Blue Straw to Staghound.

Field Command and Sandia followed up quickly on the question of a beacon on the reentry vehicles and concluded by January 11 that Cubic could not put the transponder in the vehicles; however, a transponder in one of the pods would be acceptable.

On January 12 Starbird formalized the establishment of Task Group 8.3 (Navy), Task Group 8.4 (Air Force), Task Group 8.5 (AEC Support Task Group), and Task Group 8.6 (Johnston Island Command). He also formalized the first Task Units as 8.1.5 (Space Systems Division), and 8.1.6 (EG&G).

One of the persistent troubles started about this time. The McMillan Committee was not pleased with the pods that had been suggested by AFSWC and DASA to obtain ablation data, etc., but wanted to test a genuine reentry vehicle. They thus proposed that an R&D version of the Minuteman Mark 5 reentry vehicle be used instead of the pods and asked for an investigation of this concept in mid-January. This uncertainty persisted throughout the entire planning period. By the time it came to a head, Douglas had run wind tunnel tests, etc., to convince themselves that the pods would not do anything disastrous to the aerodynamics of the Thor, but there was no way to achieve that same satisfaction for the mock RVs.

Another major change was made in mid-January. The initial agreement with Douglas and SSD had been that there would be an initial certification shot of the Thor fired from Vandenberg AFB in March. From discussions early in January it became clear that the missile trajectory should be the same for all tests, the different altitudes being achieved by firing the warhead at different times on the missile descent path. Consequently, it was desirable that the certification shot have essentially the same trajectory. Unfortunately, for safety reasons, that trajectory was not acceptable for launch from Vandenberg. Consequently, in mid-January the decision was made to fire the certification shot from Johnston Island itself on or about May 1, essentially as soon as the pad and the firing system could be ready. At the same time, the Pacific Missile Range agreed to provide the range safety service for the Johnston Island operations, using a range safety ship anchored in the Johnston Island lagoon. The fallout prediction unit was also established at this time as a result of a request by Starbird to John Foster of Livermore for Vay Shelton's services. It was Shelton's responsibility to put together that prediction unit.

On January 17, 1962, an agreement was signed by the AEC, the Task Force, and PACAF in which PACAF agreed to minimize their work on the island for the duration of Dominic. JTF-8 took control of the island on January 22.

The AEC high-altitude effort also began to take form during the early part of January. That effort consisted of several parts. First were those measurements to be conducted on Urraca as part of the development of a deep space diagnostic capability. All three Laboratories participated in that effort, but LASL was perhaps a little more heavily involved since they had gotten an earlier start and also because of their interest in the deep space problem during the moratorium and the growth of Vela Hotel and Vela Sierra. Thus, LASL P-Division, under Taschek and others, in conjunction with Sandia, began to develop detailed plans for measuring all the device outputs using instrumented rockets that would be launched by Sandia from Kauai and Point Arguello in California. They would also use Vela Sierra (surface-based) equipment deployed by AFTAC, and, if it could be arranged, Vela Hotel satellite-based instrumentation.

LASL began to define an extensive photography program with the help of EG&G. That photography would be done from a major station on Johnston Island, from Mt. Haleakala on Maui, and from a C-135 flying in the appropriate position with respect to the shot. Sandia also began planning not only for support of the other AEC laboratories, but for some Johnston Island-based photography of its own. LRL, slightly later, in conjunction with Sandia, developed somewhat different experiments aimed mainly toward the observation of x-ray and neutron outputs from the very high-altitude shots and toward deep space diagnostics. Knowing that Urraca was somewhat uncertain, all three Laboratories designed these experiments so they could also be used on Starfish and, to a certain extent, on Bluegill. By late January the concepts for these experiments were in hand, and over the next three months the effort was directed toward building the equipment, doing the construction, and moving the equipment into the field.

In conjunction with their long experience on fireball phenomena, LASL was able at that time to carry out detailed calculations of the expected device outputs and the expected interaction with the atmosphere and geomagnetic field. By the later part of the planning period these efforts had resulted in sufficiently detailed predictions that they were essential to the AEC experimenters and they were also of great assistance to Department of Defense efforts.

The instrumentation to be launched from Point Arguello was designed to measure in essentially the same intensity ranges as might be used in diagnostic methods in deep space. The closer instruments launched from Kauai and Johnston would, in general, see much higher intensities. Thus, LASL and Sandia attempted to get Journeymen rockets for firing from Point Arguello. This was eventually accomplished by a letter from a high level in the AEC to Mr. Seamans, the NASA Administrator. Vandenberg AFB agreed that the missile could be launched from Point Arguello. In early March LASL switched to the use of Astrobee 1500 rockets. Sandia would provide the Astrobees for Urraca.

Samuel and Wignall had a difficult time with the Air Force in obtaining the proper number of C-135 aircraft to be modified for both the DOD and the AEC, the Air

Force supply being very tight at the time. Initially, LASL had intended to use a C-130 for the high-altitude photography, but in early January Westervelt concluded that it would not have the proper characteristics and therefore requested a C-135. (The 130 had too much vibration and could not fly above the clouds expected.) Livermore made a similar request. On the other hand, DASA wanted two more such aircraft for similar modification, in addition to the already modified Air Force Cambridge Research Lab C-135. AFSWC got the possible users together in mid-January and concluded that LASL did have a valid requirement for the C-135. At that meeting LRL withdrew their request for the aircraft, believing that sufficient coverage would be accomplished by the other organizations. It was pointed out, however, that if AFTAC were to join the effort a fifth plane would be needed since there would not be room on any of the other aircraft for their experiments. In late January Systems Command agreed to furnish the LASL KC-135 aircraft for modification in the Big Safari (General Dynamics/Air Force Logistics Command (AFLC)) project to a configuration to be determined by AFLC and LASL. The plane would be delivered by SAC to AFLC Plant 4 at Ft. Worth (General Dynamics) on Janaury 29. SAC would also provide the crew and maintenance personnel. After modification the aircraft would be needed at Kirtland for about two weeks early in April before deployment to Hickam for participation in the Tiger Fish dry run (Thor launch). General Samuel thought he had arranged for a recent model KC-135 for LASL, but when the plane was delivered it turned out to be a vintage 1955 aircraft (Tail No. 553136) which had not been maintained in accordance with USAF tech orders. The time needed to comply forced a 23-day delay in installation of LASL instrumentation, making it unclear whether participation in the certification shot of May 1 could be achieved unless some of the tech orders were waived. This possibility was not pleasing to LASL technical people who would not care to fly in an "unsafe" plane. Hoerlin recommended that another aircraft be obtained immediately. However, after General Samuel looked into it, the answer came back from Air Force Logistics Command at Wright-Patterson AFB that they were planning to put in a maximum effort, that in no case would flight safety be compromised, and that they expected to have the plane ready at the desired time. This action satisfied CJTF-8, and LASL calmed down a bit. By March 7, with hard work, the aircraft seemed to be approximately on schedule and LASL could even offer AFTAC space in 553136 for certain of their gear. On March 14 Hoerlin thought the plane would be at Kirtland by March 31 and would be able to conduct four check-out flights and leave for overseas on April 19 or 20. However, two days later, information seemed to imply an almost 20-day delay, which would mean missing the certification, and Hoerlin so notified General Samuel, seeking assistance to get the proper support for his aircraft. He made it clear that in the first place he could not afford to miss the certification flight, and in the second place he wanted to participate in the early Christmas Island LASL detonations as a further shakedown. Apparently the pressure helped; by April 17 the LASL KC-135 was scheduled to depart Kirtland on April 21, and on April 23 Hoerlin was on Johnston Island, ready for the rehearsal of the certification shot, then to be held on April 26.

DASA had similar problems with their two KC-135s which were to be modified by the Air Force Office of Aerospace Research. Throughout February 1962 JTF-8, TG 8.4, TU 8.1.3 (WET), and SAC were trying to speed up the modifications to those aircraft.

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[redacted] However, for the fixed installations, AFTAC did have to cooperate and for many of their projects "piggybacked" on the WET programs. TU 8.4.1, in February, was commanded by Lieutenant Colonel D. B. Herbert, with alternates Major K. Mendenhall and Major J. R. Adams. They informed the Task Force they would conduct some 12 scientific projects using 30 stations established in Hawaii, Palmyra, Johnston, Midway, Maui, Kauai, Fanning, Kwajalein, Wake, Tutuila, Okinawa, Guam, and Penrhyn. AFTAC cooperated closely with the AEC Laboratories, particularly LASL, in the design of experiments to advance their technical capability for observation of foreign detonations. They instrumented to observe seismic signals from the high-altitude detonations, remote air pressure changes, remote electromagnetic signals, changes in the ionization of the air, etc. By putting some of their gear on the LASL C-135 they also could observe the Christmas Island detonations from appreciable distances, although it remained for LASL scientists to develop successful techniques for long-range observations.

LASL had yet another traumatic experience, this one with respect to their optical station on Mt. Haleakala. There was no housing at the top of the 11,000 foot mountain, and the drive down to the ocean to the existing hotels was long and, to a certain extent, hazardous, the road near the top being very narrow and having many hairpin turns. The Park Service was just closing up an old installation called the Silversword Inn several miles from the top of Mt. Haleakala, and LASL initiated steps to borrow that inn for housing for the duration of the operation. The Task Force, H&N, and AEC all got into the act; the Park Service was reluctant, but after a month or so of prodding finally agreed, and by early March the LASL personnel were ensconced not far from the top of the mountain.

The arguments concerning pods and RVs continued through the first part of 1962. Since the McMillan Committee had recommended putting the RVs on Starfish, and DASA thought it was already too late to do that, Booth made a clever suggestion in mid-January, that the RVs be flown on the proposed AEC high-altitude shot, Urraca. He continued along that vein, and in early February the pods for Starfish and Bluegill were defined as identical in total weight, external configuration, and center of gravity. Each set would have a total weight (3 pods) of 1,200 pounds. The pods would be positioned at different distances from the burst, ranging from 2,500 feet up to 14 kilometers. However, the AEC aim was to get Urraca as high as possible. When Douglas finished calculations of the initial trajectories in early February the highest possible altitude turned out to be 1,300 kilometers. Since the height would be appreciably lower carrying pods or RVs, the AEC made it clear that they did not want pods on Urraca.

Early in February, DASA decided on the division of responsibility for each of the three pods on the three shots Tiger Fish, Starfish, and Bluegill. One was assigned to the Army's Ballistics Research Laboratory (Aberdeen), one to the Nuclear Defense Laboratory (Army Chemical Center, Maryland), and one to AFSWC and ASD (Air Force Aeronautical Systems Division).

The McMillan Committee continued to press for RVs. Early in January they had met in California with representatives of SSD, AFSWC, Douglas, Convair, and Avco (the manufacturers of the Mark 5 reentry vehicle). It was clear at that meeting that Douglas could not guarantee, without further wind tunnel tests, that Mark 5 reentry vehicles placed on the Thor (instead of the pods) would allow satisfactory operation of the Thor. Furthermore, DASA and their experimenters felt fairly strongly about having some of the pod experiments on Starfish, and were unwilling to have all three positions taken by the RVs. As a result it was concluded that Douglas, in making the internal structural modifications to the Thor necessary to carry the pods, should also arrange for attachment hardware so that Starfish could take RVs, if that was decided, or a mixture of RVs and pods. If a mixture were possible, the RVs would then be positioned at 10 and 14 kilometers from the burst and the pod at 7 1/2 kilometers. At the same time it was noted that Convair was having problems developing the recovery package for the pods in time for the May 15 date of Starfish. Noting that Urraca would not carry pods and, hence, would not have a recovery problem, the McMillan Committee recommended that scheduling be changed so as to do Urraca on May 15, Bluegill on June 1, and Starfish on June 15, thus allowing time for the pod recovery package to be developed and for Avco to develop the complete RV system and recovery package. Later in the month the decision was made to build both pods and RVs and put the appropriate mounting attachments on the Starfish Thor. Douglas made it clear that they were very uncertain whether any method could be devised for carrying a mixed load. By mid-April Douglas had carried out wind tunnel tests showing that the mixed load would cause the missile to be unstable. They were working on a design to fix the instability, but had little confidence that a reliable design could be implemented in time for the June 15 firing. The conclusion was reached to continue with both RVs and pods and try to decide what to fly two weeks before the shot date, or about June 1.

The AEC Laboratories initially did not object to the May 15 date for Urraca; in fact, they were happy to get it into the schedule at all, so throughout March the change proposed by the McMillan Committee was accepted. However, Kiley, who was responsible for the DOD effort, objected on the basis that Starfish and Bluegill were of higher priority than Urraca, and, therefore, the scheduling should be done in such a manner as to maximize the probability of the successful launching and obtaining of maximum data on Starfish and Bluegill prior to June 30 (since the President was going to terminate testing on June 30). He also pointed out that this would allow the DASA projects to participate on Urraca, having completed their efforts on Starfish and Bluegill, and that this was of appreciable interest to the Department of Defense.

In early March Starbird began to be nervous about all these proposed changes in schedules, in pods, etc., and instructed the SSD Task Unit (8.1.5) not to make any further changes in weight, configuration, and trajectory without his specific direction.

Also in early March Field Command began to discuss the time of day for the detonations, noting that they needed at least one hour of darkness at the burst point for photographic documentation, but they needed daylight for recovery of the pods and RVs. They were not concerned about the lunar background. However, Hoerlin of LASL was, and in mid-March he pointed out that May 15 was a particularly bad time for Urraca since there would be a full moon within 5 to 20 degrees of its burst location, as seen from the aircraft and Maui. He also pointed out that Starfish had similar problems on its scheduled date of June 15, and requested that we have another look at the schedule. LASL also began to get in trouble because of the late scheduled delivery for magnetic tape recorders to be used in their rocket instrument packages. Taking all these problems into account, Shuster conferred with the Laboratories and DASA, and, in early April, requested that the schedule be changed to have Bluegill on June 1, Starfish on June 15, and Urraca on July 1. Starbird checked the point with his deputies and accepted the proposed change.

In the early months of 1962 problems arose with the orientation of the RVs carrying the warheads. The Atlas RV chosen by Sandia much earlier used a very heavy copper heat sink to assist in reentry. It was quickly realized that the large mass of copper would affect the device outputs and appreciably change the phenomena. These

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effects would be especially serious on the higher-altitude shots Starfish and Urraca, which would not engulf large masses of air in the early part of their expansion, but were not so serious on the comparatively low-altitude Bluegill. [redacted]

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[redacted]2 went by Ogle asked several people to help him on the multitudinous detailed safety problems that had come up day by day. One at a time, the various people were asked to help in given fields and the conglomeration of such people eventually became known as the Hazard Evaluation Group. Although they were never formally organized during Dominic, the membership kept changing as people had to move around the Pacific for different efforts. Their work would have been easier had they been recognized formally.

In December 1961 Starbird had become quite worried about the safety of the airdrops and had appointed an ad hoc safety committee to review in detail the drop procedures, firing and fusing, etc. In late January AFSWC noted the existence of this committee and inquired if anyone was worrying about the same kind of problem on the high-altitude shots. As a result, in mid-February, Rod Ray, with Starbird's concurrence, set up the Fishbowl Safety Committee to review the fusing, firing, range safety, etc., for the high-altitude shots.

In late January Ogle asked the Laboratories whether their devices would go critical if they fell in the water, and how did the result change at great depth. After some time, the conclusion was that the devices would not go critical.

As mentioned elsewhere, in January and February Van Dorn of Scripps was funded by the AEC to assist the Task Force in water wave prediction and also to instrument a number of spots in the Hawaiian Islands to measure any tsunami formed. His work was continually watched by Ken Olsen of LASL. In late March Starbird appointed Lieutenant Commander P. Kwart as his Project Officer for Range Safety of the Thor launches from Johnston Island. A Range Safety Officer would have authority to destroy a missile, but any warhead destruct command would come directly from the Task Force in the Operations Center. In March it was decided to store the extra rocket motors on Sand Island, part of Johnston Atoll, rather than on Johnston itself. The motors could not be stored in Hawaii because they couldn't be transported to Johnston soon enough after one shot to prepare for the next shot on the required schedule.

On March 15 Starbird informed TG 8.4 that they had the EOD (Emergency Ordnance Disposal) responsibilities for the whole Task Force, and that they should establish appropriate capabilities at Barbers Point, Christmas Island, and Johnston Island during the operation.

In mid-March the question of the birds on Sand Island of Johnston Atoll was considered. During Hardtack great efforts had been made to prevent damage to the birds, including building a water spray system and laying down an artificial smoke layer over the island. [redacted]aking into account the Hardtack experience, it was concluded that no protective measures needed to be taken for Dominic.

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However, the greatest problem with the expected effects of the high-altitude shots was that of possible eyeburn if the detonation should be viewed directly. Substantial work had been done on this problem during Hardtack and it was because of this hazard that the Hardtack high-altitude shots had been moved from Bikini to Johnston. Dominic presented a new problem, since these detonations would expand in what amounted to a vacuum. The approach adopted had two phases: one was detailed calculations by LASL and DASA on the brightness and light flux to be expected, and the other, funded by DASA, was discussions of the characteristics of the human eye with respect to burns, largely using the data of Dr. W. T. Ham. On March 16 Hoerlin commented on the Starfish assessment:

> A rather shaky extrapolation of Ham's data indicates that for small image sizes, burn threshold is near one calorie per square centimeter. Consequently, use of dark glasses in danger area must be recommended. The Hawaiian Islands appear safe. However, the more competent opinion of medical biological scientists is solicited. . . . After 100 microseconds, the brightness of debris drops drastically and its contribution can be neglected in view of the somewhat generous integrations of early dose.

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5 U.S.C. 553 (b)(3)
DOE, Exemption 3

Debates on this problem, which continued almost throughout the whole operation, determined the size of the danger area in some cases. The hazard to people in high-flying aircraft was such that for some shots the danger area at aircraft altitude extended beyond the Hawaiian Islands. At the end of March Ogle concluded that the [redacted] shot at 11,000 feet (Frigate Bird) should be conducted so that no uncontrolled observers were within 150 statute miles. For Bluegill, some 470 nautical miles would be the required stand-off distance. Starfish and Urraca appeared to be safe for viewing from Hawaii, but between Hawaii and Johnston people on the water should be required to wear dark glasses.

On Johnston Island itself, assessment of potential hazards led to the conclusion that in order to protect people from rocket misfiring no one should be closer than 2,500 feet from the Thor launcher, and those people either had to be in underground shelters or in specially designed facilities. Only essential personnel would be allowed within 1,000 feet of any small rocket launchers, but they could be no closer than 500 feet and had to be behind suitable sandbag barricades at launch time.

Arrangements by the State Department, JTF-8, etc. continued during the first four months of 1962 to obtain land in many foreign possessions and many small islands for locating both experiments and weather stations. Equipment was prepared and shipped to the islands as soon as permission was obtained.

On March 17 arrangements were made to set up a special weekly flight from Hickam to Palmyra, Canton, Viti Levu, Tongatapu, Tutuila, Rarotonga, Tongareva, and back to Hickam. Another weekly flight was arranged from Hickam to Kauai, Niihau, Maui, French Frigate Shoals, and back to Hickam.

While the formation of the Task Force was publicly announced on March 2, some restrictions were maintained for the next two weeks, but on March 17 Starbird informed the Task Force Units that they would now change their cover stories and admit that they were engaged in authorized preparations to resume nuclear testing in the atmosphere, should it become necessary.

By March 26 there were 1,000 people on Johnston Island. The users had occupied the missile launch facilities, arrangements had been made to tie the Range Tracker to the dock, and plans were being made for helicopter evacuation of personnel to an aircraft carrier before shot time. On the negative side, the sewage system was overloaded and serious problems existed with the sanitation facilities, the distillation plant was having troubles providing enough water, and the Island was crowded. Starbird requested that no more projects be added.

On April 6 Joe Sanders replaced Reeves as Commander of Task Group 8.5. Requirements continued to be added. In mid-February, because of the problems experienced on Hardtack in maintaining communications with Hawaii immediately after the shot, the Task Force requested that several aircraft (preferably B-47s) be made available for direct line-of-sight radio relay between Johnston and Hawaii in the event of high-frequency blackout. On March 8 the need for still another aircraft was noted to help calibrate the Cubic Corporation's tracking equipment on Johnston; it would carry a beacon and fly around Johnston.

As shot time approached other problems arose. In late March Shuster, who had been designated acting Scientific Deputy on Johnston Island, was asked to compile a complete "go-no-go" list for each experiment connected with the high-altitude shots, based on inputs from each Task Unit, and have it ready no later than April 5. The list was not quite ready then, and, of course, was still being debated when we were told to start testing.

On April 12, with the nation somewhat disturbed at the size of the danger areas, especially at aircraft altitudes, the Commission discussed the question. The FAA had agreed that they could route aircraft around the danger areas, given two days notice, but they would prefer more. The uncertainty of the weather made a longer notice somewhat questionable. It was agreed that a four-day notice would be given to FAA along with a similar warning to the Hawaiian officials. Other points came up in this same meeting; in particular the question of U.S. citizens attempting to interfere with the operation. It was agreed to issue a regulation which would allow the Commission to seek an injunction if someone tried to enter the danger area.

Of somewhat more importance at that meeting was the discussion of visits by U.S. and non-U.S. VIP observers to Christmas and Johnston Islands. Starbird felt that "It would be entirely wrong to have non-U.S. personnel at the Johnston experiments. Not only are the phenomena awe inspiring, but it would be almost impossible for us to enforce the necessary classification control aboard the evacuation ship from which they would view the shot." However, U.S. VIPs' trips were arranged for late in April and late in June on Johnston and late in May for Christmas Island.

By April 24 pressure from the McMillan Committee and DASA was already resulting in a move to add to the number of shots for the high-altitude portion of Dominic. Kingfish was being planned and DASA was beginning to make plans for the backup Thors.

By April the Spain Committee's instrumentation chart for Dominic, including just the listing for the projects with a brief description, was 60 pages long.

By April 24, although the date for Tigerfish was near, there were still problems with the pods. The Tigerfish dry run was scheduled for the 26th, the word was out that additional events might be added to the high-altitude series, the long-range countdown system was now transmitting half-hour practice runs twice daily, and the island was ready for the President's announcement.

The Resumption of Testing

On April 5, 1962, Starbird had declared the beginning of the operational period, at which time, the JCS had noted, he would report directly to them rather than through DASA. On April 24 Starbird was told by Betts to go ahead. It is not the intent of this history to give all the details of the operations on Christmas Island, etc., but some of the major points will be discussed. When the operation began we were on one-day notice for the Christmas Island shots, were ready to do the high-altitude calibration shot Tigerfish on May 1, were intending to do the Polaris system test shot on May 5, were performing rehearsals of the ASROC systems test, and were still arguing about the Atlas systems test.

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The Christmas Island Operation

The Christmas Island operation went smoothly, though not without technical problems. [redacted] There were, of course, operational and, as noted, technical problems. The organization worked extremely hard on the first 12 shots and complained bitterly when things slowed down in late May because of a lack of devices to fire. The people on the island concerned solely with Christmas Island operations nevertheless were pleased and, toward the middle of the operation, comparatively relaxed. However, the senior Task Force staff who also had to worry about four other operations were rather harried.

Several lessons were learned, and the first one came very hard. Ogle and Aamodt agreed to fire the first shot in a cloud and, consequently, lost the fireball data. That taught us to be more careful, and a judgment system to observe the clouds and determine when a hole was coming by was quickly set up using a crew of several people at the forward A Site and using data from the B-50 weather airplane flying upwind. A detonation in clouds did not happen again, but after about three weeks of firing, LASL raised the height of burst to get above the low level clouds.

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Another technical lesson was learned early in the operation. Fireball measurements from the C-130s were not worth the film they were printed on. There were all sorts of troubles, but the major ones were with the distance measuring equipment. By the time the operation was about half over, good data were obtained intermittently, and the reason for the problems began to be clear. Thus, by the end of the Christmas operation, the C-130s were turning into moderately useful tools and the technical organization knew what to do to make them better.

At a March 28, 1962, briefing of the Atomic Energy Commission by the Joint Task Force, Ted Parsons had explained the details of the B-52 operation. The B-52 planes would depart from Barbers Point on Oahu, fly to, but not over, Christmas Island and proceed south of the island into a 16-minute racetrack pattern. The aircraft would then proceed on four test runs to ensure that its course remained within a 6-degree cone of the intended area of detonation. The B-52 would receive the final signal to release at minus nine minutes; at this point, the manual bomb rack would be unlocked. At minus one minute, the device would be armed and the bomb bays opened. Either the arming of the device or the unlocking of the manual release rack would be delayed until at most one minute prior to drop. This precaution was necessary because a large portion of the experimental team was at A Site, which had been placed as close to the detonation as was safe from the point of view of blast and thermal radiation. Parsons noted that each test would have three primary means of control, any one of which could abort the mission. The first control required that the bombing aircraft remain within a 6-degree cone on its final racetrack run, as determined by its own radar. The second means of control was at the Air Operations Center (AOC) located on Christmas Island, which also monitored the path of the plane to ensure that it remained within its proper run (by this, Parsons meant the information supplied from the C-121 control aircraft to the AOC). Finally, the Sandia Corporation had established an independent radar network at another location on the island. Each of these systems was, in theory, capable of halting the countdown. The difficulty was that the Sandia system initially was not capable of halting the countdown, both for administrative and technical reasons. The technical reason, at least for a while, was that proper communications with A Site were not possible. The administrative reason was that the Air Force wanted to control their operation and did not want to depend upon a civilian system for safety considerations. However, that attitude disappeared very early when the bomber, on one of its early orbits, lined up on A Site instead of the target. There was a great amount of screaming from the people in A Site and things were straightened out quickly, but from then on the Sandia radar was an integral part of the safety system.

Because A Site was so close to the explosion, the people there were required to wear long-sleeved shirts and/or cover themselves with white sheets if they were to be outside at shot time. The thermal radiation had been carefully estimated and found to be such that it might occasionally be slightly painful, but would not cause serious burns. On some shots the temperature inside the sheet got fairly high, but no one was hurt from this effect in the entire operation. Furthermore, there was no serious damage to equipment at A Site from blast, although on a few shots the somewhat more flimsy structures at the JOC were shaken up pretty badly.

The natives, however, were not quite so hardened as the Laboratory people, and as the shots went on some of them began to be a little frightened. Early in May a number of the native wives had approached the District Commissioner, expressing a desire to be evacuated with their families to Fanning Island. AVM McKinley and the Commissioner met with the groups and explained that there was no cause for alarm. Some of the natives could not exactly define their difficulties. Some were basically afraid and others indicated that the early rising and tensions during the countdown upset them and their children. [redacted] Consequently, the Task Force arranged that those who wished could be taken aboard the LSD Cabildo, either the night before, if the shot was to be very early in the morning, or early in the morning if the shot was to be later in the morning. Aboard ship they were shown a movie and given tea and bread, and they were returned to the island after the shot. In general, about one-half of

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the natives went aboard the ship and the rest remained ashore. Unfortunately, the U.K. authorities in Britain became aware of the problem and suggested to Gerry Johnson that the U.S. provide sealift for all the natives to Tarawa, but this never became necessary.

There also was a bit of a problem concerning the pay for the native male workers because we were disrupting their work. [redacted] Starbird had been approached by McKinley with a suggestion that the U.S. pay the native male workers $3.60 per month dislocation allowance for the full three-month period of the Dominic series. Betts agreed that this could be paid out of AEC test funds.

The shots were occasionally delayed for weather, which, in the initial part of the operation, led to a great number of tired people. Fortunately for TG 8.4, there were backup crews for the B-52s. An example was Arkansas. On April 30 Ogle reported:

> **We've tried Arkansas (LRL) twice now. Clouds prevented shot morning of 28. This morning, the 10,000- to 20,000-foot winds went over the island. The cumulus do occasionally reach to that altitude (i.e., 12,000 to 16,000 feet) and some rain might be expected from them. The prediction of rainout at London (Christmas) was uncertain, might be several roentgens. This is too early in the operation to do that. We will try again tomorrow.**

In the earlier operations at Eniwetok there had been a great deal of difficulty with visiting VIPs. The problem was that these were all moderately high-level people, such as the Secretary of Defense, and they had to have a place to sleep, but there was no place to sleep on the island except in beds that, in principle, already belonged to some of the working people. Therefore, in those early operations the working people, usually rather high level in the organization, were moved out of their beds for the duration of the visit and were generally pretty bitter about it. Their view was that the visitors had nothing to do there particularly, were usually on some sort of a boondoggle, and were interfering with the work when such a move was required. (Of course, the visitors never requested this, but the Task Force hosts felt it necessary.) Because of this perennial problem, a new plush barracks had been built on Eniwetok for Hardtack just for visitors. This turned out to be a very satisfactory system. The visitors were happy and the staff was happy. Therefore, when we became aware early in 1962 that we were going to again be host to a great number of visitors, Ogle recounted this experience to Starbird and Starbird ordered a supply of house trailers (nicely built) for the visitors. The house trailers arrived in time and were set up and again the visitors and staff were happy.

Visitors would not have been happy, however, had they been required to eat in the mess hall. The problem of making pleasant eating surroundings at the Main Camp was never really solved, although it did get better toward the end of the operation. The basic trouble was simply that the facilities were too small for the number of people involved. However, the Navy had arranged for a very pleasant officers' mess which turned out to be very useful in hosting high-level visitors.

As noted earlier, the British made diagnostic measurements on our Christmas Island shots with our agreement. The Russians also made measurements with no formal agreement. During most of the operation a highly instrumented Russian ship stayed just outside the danger area, watching our shots. They would occasionally go to the Johnston area for those detonations. The Task Force and the Commission discussed the subject, but there was really nothing we could do about it except watch and keep fairly close by to make sure that they did not come within the danger area. During a visit to a number of the small island sites in the South Pacific, Starbird had a stopover in Fiji. While sitting in a small restaurant, he and his party observed that the Russian ship Captain and his party were sitting at an adjacent table. Starbird did not want to chance an international incident, and therefore, did not have a discussion with the Captain. Luedecke, in discussing the problem of Russian presence at the tests with Gerry Johnson, said:

> A meeting of an ad hoc group on security of U.S. testing was held on March 30, and the results have been reported to you. In summary, discussions indicated that very little could be done at this date except to initiate certain contingency planning along military lines in case the U.S.S.R. takes overt steps to interfere with the Dominic series. It was also agreed that counterintelligence planning action should be initiated for future series beyond Dominic.

Another flap arose that is worth commenting on. In most previous Pacific operations at Eniwetok and Bikini there had been a strict rule against the use of private cameras. However, at Crossroads cameras were furnished to a great number of the personnel, and they were encouraged to take any pictures they wanted to, with the idea that out of that mass of film would come a good documentation of the operation. In actual fact most people are not good photographers, at least not with the equipment as it was in 1946, and almost all of the pictures were of no value. However, since they were practically all unclassified, they did serve as good mementos to the people involved in that operation. Remembering that, Ogle suggested to Starbird in early 1962 that we allow Task Force members on Christmas Island to use their own cameras to take pictures of anything they liked, just asking them not to take pictures of anything classified. Basically, the argument was that there was nothing classified, unless a document was copied or something of that sort. Starbird acceded to that request and for the buildup period at Christmas Island, and into the operation a little way, such private cameras were allowed. Unfortunately, there is always someone to spoil things, and, in this case, a picture of a detonation was sold to the Oakland Tribune Publishing Company and appeared in many of the Sunday newspaper editions on May 27, 1962. President Kennedy had a phobia about public exposure and specifically did not want to have pictures of bombs going off appearing in the newspapers, so the lid was clamped down immediately. The next day all private cameras were picked up on Christmas Island, to be held for later return, and all personnel were required to sign a certificate that they had turned in all nonauthorized cameras, film, and prints. Along with other directives to the members of JTG 8.4, Samuel stated:

> When the initial policy on cameras on Christmas Island was established, it was done so under the then existing criteria of what constituted classified and unclassified photography. These definitions have not changed. However, the sensitivity of the Christmas operation had not been anticipated. Unfortunately, because of a few of our people who, for whatever reason, will take into their own hands issues of this kind, it has been necessary to inflict on most of us considerable inconvenience to say the least. Unfortunately, we are forced to take positive steps to preclude the weaker among us from succumbing to the temptations which in the best judgment of those in authority can result in national disadvantage. I sincerely regret the circumstances which make this action necessary and assure you that we are doing everything possible to protect the equipment and photography.

In late June Carolyn Carlson, a physicist in the Livermore test division, wrote to President Kennedy complaining about the exclusion of women from participation in the test series. She noted Kennedy's numerous statements on nondiscrimination and concluded, "The current laboratory policy responding to the traditional military reluctance to treat trained women as professionals is unwarranted on the basis of either the ethical or the practical considerations involved." The letter was referred to Seaborg, who stated he would include the consideration of women in appropriate facilities in planning for future testing overseas.

The Air Force Aeronautical Systems Division finally managed to do the B-57 thermal experiment mentioned earlier. In order to get the proper flux, the aircraft had to be very close to the detonation point at zero time. The aircraft was moving quite rapidly, and there was a long series of arguments between the project people on the one hand and Starbird and Ogle on the other hand concerning the placement of the aircraft. (The aircraft was controlled from radar on the ground.) However, eventually the debate was resolved and the aircraft participated, with good results.

The last shot at Christmas was the LRL Pamlico event on July 11, 1962. While Americans did not completely leave Christmas Island until late 1963, by midnight of July 11 the technical organization had rolled up to such an extent that it would have taken a month to get ready again. However, several steps to preserve the capability had already been taken. By late May the budget cycle had progressed such that the Laboratories were making proposals for future test operations. Ogle argued to try to keep Christmas Island as well as to develop the capability for completely airborne diagnostics, implying that new aircraft such as Boeing 707s should be obtained and that a satisfactory DME capability should be developed. AFSWC also argued to maintain the airborne capability but suggested three C-130 aircraft.

However, by early July Air Force Headquarters was requesting that the aircraft be returned after the end of the Christmas Island portion of the operation. Samuel objected and pointed out that, considering the high quality of data obtained from the aircraft after a very short time of preparation, the probability was high that given another 11 months (to the next operation), the aircraft would become quite satisfactory data-collection platforms. He went on to argue that if the C-130s had to be lost, then at least one more C-135 should be added to the Task Force resources of test aircraft. The Air Force suggested a modified C-97 and Samuel objected, pointing out the high record of reliability of the C-130s which would not be likely with C-97s. (C-97 is a piston engine aircraft, whereas a C-130 is a turboprop.) The Laboratories saved the day by requesting early in July that the C-130s be kept for use against the high-altitude shots in order to make electromagnetic and optical time interval measurements. After some discussion, the Air Force agreed.

Meanwhile, Ogle had pointed out to Bradbury, Foster, Betts, and Reeves that there now appeared to be no political resistance to using Christmas Island as part of Dominic, but the high-altitude shots were causing great political flurry; therefore, he suggested that one solution was not to stop testing at Christmas at all (as of July 8), but try to continue to do a shot every couple of weeks or perhaps as little as once a month, fitting in with the development plan for the Laboratories, and hence keeping alive and nourishing the atmospheric test program at Christmas. Senior representatives of both Laboratories, who happened to be in Los Alamos, discussed the question. Their reaction was that the Laboratories jointly could not provide that many shots in the near future, and that they would prefer to prepare an orderly, well-planned, atmospheric test series for the fall of 1963, feeling that by that time a very satisfactory open sea operation could be developed, and, hence, we would not be dependent on Christmas Island. They suggested that the outer space capability be developed and perhaps demonstrated the following year. Perhaps more pertinently, they asked, "What do we use for money to operate Christmas Island and still operate NTS?" Betts advised Ogle to go ahead and roll up the island.

It is to be pointed out that the failures that had by then occurred at Johnston Island in a way helped the Christmas operation. Kennedy had authorized several of the later tests only if they were ready by the end of the high-altitude operation. Since that operation stretched on in July, two shots, Sunset and Pamlico, were approved and fired at Christmas.

A summary of the Christmas Island technical results, given in Appendix A, is a somewhat edited version of the "quick look" report prepared by Al Embry of LASL immediately after the operation. (Ed. note: Because the author had not completed his editing of the Embry report by the time of his death, we have chosen to include as Appendix A only an abstract of the general results part of the Embry report. The complete report will be a part of the William Ogle Collection of papers maintained in the archives of the Los Alamos National Laboratory.)

Follow-on Air Drops

During the evening of July 25, 1962, a Thor was destroyed and burned on the pad on Johnston Island. There came then an approximate two-month interval of no testing at Johnston Island, which allowed the Laboratories to think a little bit more about their problems in developing high-yield devices.

In mid-July 1962 Air Force Headquarters had initiated attempts to get the B-57 samplers back, but AFSWC resisted, arguing their need for the upcoming 1963 operation. AFSWC also argued that by now the C-130s had been altered so much that to remodify them for normal use and then reconfigure for a later operation would be an unreasonable cost. However, by the early part of August the Air Staff had judged that Air Defense Command need for the B-57 aircraft as high-altitude vehicles had priority and four of the B models were returned, leaving six in sampler configuration.

The break in the operation due to the July 25 accident on the pad at Johnston Island apparently induced Betts to think about continued atmospheric testing. On July 27 he asked Reeves to estimate the AEC costs and DOD effort to support continual atmospheric testing at a rate of one or two shots per month, using either completely airborne, or airborne plus supplemental surface, diagnostic measurements. He assumed the continued availibility of the airborne diagnostic system and asked that various areas of operation be considered. He discussed the question informally with Bradbury and Foster and requested their reactions.

On August 2 Foster advised Betts of the LRL desire to conduct further atmospheric detonations during Dominic [redacted] He went on to note that the Russians had announced their intention to conduct further atmospheric tests during August, September, and October and said, "The Laboratory should make every effort to prepare to test their most useful and urgent experiments." He added that LRL was starting the design and construction of [redacted] devices. Bradbury replied to Betts on August 8 that:

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> LASL has considered with care your informal request concerning our possible interest in additional experimental shots in the Dominic program in a time scale of the next two or three months corresponding with the resumption of activities at Johnston Island. We remain of the opinion that a preferable course of action would be to inform the Laboratories from a suitably high level that definite planning and preparation should be carried for atmospheric test operation in approximately one year. It appears to us that the only real arguments for additional Dominic tests at this time arise out of a fear that atmospheric testing will soon be discontinued. You will recall the many discussions in Washington last fall in which the difference between the U.S. course of action after August 1958 and the presumed U.S.S.R. course of action during this same time were kicked around. You will also recall that it was fairly obvious that the U.S.S.R. scientists had been told to plan on an operation and we

> had been told the exact opposite. It seems to us extremely important that we not forget this lesson and that it is more important that we get a general long-range policy decision than that we add a few hasty and inevitably not very well considered experiments in a rush-rush time scale on the basis of a fear of what might not happen. [redacted] Any device we could possibly prepare in the prescribed time scale would be a very ragged affair, far from optimised, and of problematical behavior. We would not recommend its testing at this time, but could regard an initial version as a very appropriate candidate a year from now following adequate calculational study. . . . It would appear to us that the only justification for trying to get bits and pieces together in the suggested time scale would be on the basis of early word from you that we should act on the assumption that further atmospheric testing in the next year or two is quite unlikely.

Bradbury went on to discuss other difficulties, finally remarking that if an operation were turned on, he would propose to repeat as an atmospheric test [redacted] His message ended with the following:

> . . . being old, tired, and sour, and the writer cannot forebear to notic [redacted] Whither are we drifting when a jaundiced observer looking at the overall behavior of U.S. missiles might easily conclude that nuclear weapons were about 10 years ahead of their corresponding delivery systems. In any event, LASL does not feel that the world will come to an end if we do not do other than spend a year studying what happened in Dominic, experimenting in Nevada, and preparing as good designs as we can in the light of the nation's needs for testing in the Pacific a year from now.

[redacted]

The test system moved rapidly. Livermore had informed the Air Task Group at Kirtland of their intentions even before they sent the message to Betts, and on August 3 TG 8.4 knew what airplanes they needed; these were virtually the same as those used at Christmas Island, including the two C-130s, eight B-57 Bs, two KC-135s for sample return, B-52s, etc. TG 8.4 notified Air Force Headquarters that the C-130s and B-57s were absolutely critical to the success of the renewed test operation and said, "In view of the compressed time schedule for the proposed operation and to prevent their unrecoverable loss, CJTG 8.4 is retaining control of these aircraft and assets." On the same day JTF-8 Headquarters in Washington considered the need for WB-50 weather reconnaissance aircraft and concluded that they would be required.

On August 4 TG 8.4 requested the return of the C-130s to Kirtland for modification of the currently installed diagnostic gear. The modification would take some time and the crews would not have to remain with the aircraft during the modification period.

Planning moved rapidly and as a result Ogle sent the following TWX on August 17:

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As a result of a meeting on August 15 in Albuquerque with representatives of LRL, LASL, DOD, and EG&G Task Units, as well as PMR and TG 8.4 and 8.5, and further discussions at LASL and Kirtland, the following concept for the airdrop operation was agreed to:

1. [redacted]

2. Nuclear devices will be assembled and loaded on the B-52s under the supervision of Lee Hollingsworth. Loading will take place at Travis or Kirtland, depending upon the Laboratory involved. 8.4 will investigate the advantage of doing all loading at Kirtland. The device checks will go down to about 5-hour readiness, after which the B-52 will proceed to Barbers Point where the final telemetry checks will be made and the DME units in the other array aircraft calibrated with the device. The key will be put in at Barbers Point. The check at Barbers Point will require a minimum of personnel and equipment, eliminating the necessity to reestablish the entire Barbers Point mechanical and electronic setup. It will be necessary to have the C-130s at Barbers Point for the calibration of the DME, but they need not be based there. The B-52s would be based in the Hawaiian area. 8.4 and Hollingsworth will arrange for the necessary facilities at Barbers Point and Hickam.

3. The Command Post for the air operation would be in an RC-121-type aircraft in the air array. This aircraft would serve as the Command Post for the senior Task Force Officer and the senior laboratory representative from the sponsoring Laboratory. It appears logical that the airborne AOC also be in this aircraft. From this aircraft, then, the Commander and the senior scientific representative would be able to control the air array and determine the readiness of the instrumented aircraft. The results of the prerelease checks and the bomb functions as obtained from telemetry would be funneled into this control point from the C-130s and B-52s by voice relay.

4. In order to obtain the best EM signals from the device, the B-52 would fly in a north-south path so that the C-130s can fly either east or west (magnetic) from the drop aircraft. As the instrumentation of the C-130s is on the left side of the planes, it will be necessary to either have the 130s on opposite racetrack patterns or else stack them and have them on the same racetrack. (LRL has requested another camera platform for their shots. If there is another aircraft in the array, then stacking the 130s is an acceptable solution. If this other camera platform is a B-52, then it would be preferable to have the 130s on either side of the burst.) It is proposed to detonate the devices somewhere between 8,000 and 14,000 feet. This altitude was selected because of altitude restrictions on the 130s and predicted weather conditions during September and October. [redacted] would be detonated in the vicinity of 19.5° north, 164.5° west, approximately 400 nautical miles southwest of Hawaii and within the Bluegill danger area. Flying in this area would permit the B-57s to return to Barbers Point directly without having to land at Johnston Island. It also permits the use of a B-57D as a controller. Even with a surface burst, this area is safe in all respects for the LASL device. LRL devices should be detonated in the vicinity of 12.75° north, 171.5° west. This area is approximately 250 miles southwest of Johnston and would require the B-57s to land at Johnston upon completion of their sampling missions. Because of the distance from Johnston, it is possible to use a B-57D as a controller by staging the plane through Johnston prior to the mission. The drop areas selected are not to be considered exact; the array will be able to move about to some degree to take advantage of favorable weather conditions. If 8.4 prefers, the last shot could also be fired in the latter area.

5. Control of the array will be accomplished using radars in RC-121 and Task Group 8.4 aircraft controllers. All aircraft will be positioned using clock time zero positions and the predicted burst point. The necessity of target rafts will be settled by discussion between 8.4 and JTF-8. Difficulties of mooring would be encountered because of the depths in the target areas. Further, the flexibility of an all airborne operation would be lost if tied to a specific target in a single area. The concept outlined here allows the array commander to shift to some degree, taking advantage of clear areas. Insofar as all airdrop operations are expected to be nighttime operations, all positioning will be accomplished by radar, and Task Group 8.4 is capable of providing this positioning control without the use of target rafts. Provisions for additional aircraft participation must be made. It is possible that the LASL and AFTAC KC-135s may participate.

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6. Additional effort will be necessary to provide adequate communications to the entire air array, as well as communications to Headquarters, JTF-8, on Johnston Island and a scientific net within the air array. This latter net will be necessary in order to pass certain information of readiness and bomb functioning to the scientific representative in the command aircraft. In addition, some sort of countdown net will have to operate to assist aircraft outside the immediate array in positioning themselves. A Motorola-type VHF or UHF net might satisfy the scientific communication requirement. Johnston-Barbers Point communications must be provided. 8.4 will arrange for the inter-aircraft communications. The drop plane countdown should be available in all experimental aircraft. This is to be arranged by 8.4.

7. The C-130s in the array will be equipped almost as they were in the Christmas Island operation with the exception of the DME. Time interval measurements and bhangmeter recordings will be taken as before. Certain modifications to improve the fireball photography are being planned, and in order to take advantage of this photography, certain major changes are being made in the DME systems within the array. Mutual agreement was reached at the Albuquerque meeting to eliminate all so-called slave stations in the DME system, and to provide direct measurements between the device and the various aircraft only. This system is contingent on the ability of Sandia to install the necessary transponders in the device cases. DME systems will be provided as follows: from device to 298 (LASL C-130), Sandia and AFSWC; from device to 299 (LRL C-130), AFSWC and LRL; from device to third camera station, Sandia. The LRL DME system is a development item and utilizes a microwave pulse technique rather than the phase shift system used by Sandia and AFSWC. The installation of additional DME systems is necessary to ensure adequate distance measurements, although it will require considerable effort on the part of the AFSWC test division to modify the aircraft. AFSWC presently has a C-54 modified for aerial photography of this type and efforts are being made by Task Group 8.4 to obtain the use of this aircraft. The modification of the B-52 camera station in the tail to accept more suitable cameras and to provide surer functioning may involve more effort than time will allow but is preferable to the use of the C-54. EG&G and SWC are investigating this now.

8. Sandia has agreed to furnish to each C-130 release and arm baro signals. In addition, Sandia will provide a real-time presentation of the bomb functioning in aircraft 298, using the standard telemetry system from the device. This information will also be relayed verbally as needed over the scientific net to the command aircraft, using a simple, prearranged voice code. An FM/AM fiducial marker will be provided to the aircraft 299 to satisfy LRL requirements. EG&G will install the proper timing systems in the aircraft to provide the necessary timing signals to the various instrument stations, as has been done in the past. It is assumed that EG&G will have facilities in Honolulu to process the necessary photography records resulting from the airdrop operations.

9. The danger area for the airdrop operation is assumed to be the same as announced for Bluegill. This should cover the problems of eyeburn to transient aircraft in the normal commercial airlines and should provide adequate protection from fallout for ships. This will be studied further. It is assumed that the major weather functions will take place on Johnston, using already established procedures and facilities. The hazards evaluation group will operate from Johnston. It is felt that these groups should be where they can advise CJTF-8 directly as to the feasibility of a given operation.

10. With this concept of operations, certain personnel will have to be deployed forward to provide for both normal and emergency functions. Some personnel will have to be stationed for short periods on Johnston to remove samples from the B-57s after the LRL shots, as well as to provide personnel decontamination and perhaps area decontamination in the aircraft parking area. Provisions will have to be made by CJTF-8 for EOD personnel at Barbers Point as well as adequate fire-fighting personnel and equipment.

11. Because of the parachute retardation system being used by LRL in their events, information concerning rates of fall and drift must be provided to Task Group 8.4 and other interested groups by Sandia so that safety studies can be initiated. It is contemplated that the drop aircraft will fly as low as possible consistent with safety so as to minimize the drift of the LRL devices. 8.4 will determine the safe separation distance for experimental aircraft. Both LASL and LRL will determine optimum heights of burst for their devices early and disseminate this information widely.

12. Rehearsals for the airdrop operation are planned as follows: 30 August, off the coast of California with the B-52, 130s, and the command aircraft only (date may change to allow aircraft modification); 10 September, same aircraft, with DRM 4 drop; 14 September, complete array using a Mark 36 case (retarded), conducted at the LASL drop area, and staging from Barbers Point. The first airdrop is being planned for 16 September. [redacted]

13. Comments to this concept and schedule are invited. I propose that Walt Dumas will assist me in coordinating the airdrop operations. Send comments to either of us at LASL. Warmest regards.

Further agreements as to responsibilities were quickly made. Austin McGuire would be in charge of the LASL portion of the airdrop operations, Robert Goeckermann in charge of the LRL portion of the operation, John Eckhart in charge of the Sandia portion of the operation, and Lee Hollingsworth would coordinate device check-out and loading for all airdrops.

Meanwhile, the airdrop system had dispersed and 8.4 had lost a great number of their people. On August 20 they asked Samuel to arrange for the recall of a large number of Air Force people by name in order to ensure success in the operation. By August 20 the approving system had written letters to the President requesting authority for the extended airdrop operation [redacted]

On August 27 the population of Los Alamos personnel in the Pacific was two.

Changes in the plan were worked out over the next six weeks. On August 24 Betts authorized preparations toward the airdrop operation and on August 30 expanded that to include procurement actions, laboratory effort, construction orders, and aircraft modifications.

During the last week in August and the first week in September, CTG 8.3 and CTG 8.4 (Mustin and Samuel) agreed that they needed a target for proper array positioning. Parts were available to put together target rafts such as were used at Christmas and deep sea mooring equipment was available. However, it then was necessary to decide where the targets should be. [redacted] That put it a little further away from Johnston than was convenient to Samuel for aircraft operation, and it made the B-57 job harder, since these aircraft would have to stage out of Johnston. Also, it introduced possible scheduling trouble with the RC-121 control aircraft which were staged out of Hawaii, since they also had to be used on dry runs for the forthcoming return to high-altitude testing. The point was eventually settled in favor of the safety arguments. [redacted]

While the AEC Task Units worked hard during August and the early part of September to rebuild the gear in the C-130s and, in particular (between themselves and AFSWC), to get the DME gear working properly, they and Hollingsworth also had a great deal of trouble because the program kept changing. LRL's original proposals of variations [redacted] seemed to be acceptable, but Bradbury's oralloy shot to improve the understanding of underground testing could not be sold permanently. He complained, with tongue in cheek, that apparently one had to have a previous failure on a given device to be allowed to get it in the fall operation.

By the end of August the system was tentatively planning, at the request of the AEC, on Androscoggin; [redacted] The

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difficulty was very clear. The President wanted the operation over by November 1 and the DOD had proposed some more high-altitude shots. Consequently, approval of the airdrops had to be mixed in with the high-altitude problem, taking into account Washington's idea of the Task Force capability to do them all, and worrying about total overall radioactivity produced. [redacted]

[redacted] These concerns led to an early September meeting during which the question of drogue parachutes was discussed by Samuel, Ogle, Goeckermann, and the rest of 8.4, the final conclusion being that a reefed drogue would be satisfactory.

Bradbury kept arguing for Tocito, but the Commission itself would not buy the shot, largely because it was too small to be included in the overseas operation. However, the test organization kept it in the schedule right up to the end, just in case Bradbury should win. By mid-September the air array was ready to go again and a first dry run was held on the 13th near Clovis, New Mexico. It looked promising. However, the second dry run the next day was aborted due to difficulties with both the AFSWC and Sandia DME systems. A third dry run on the 17th, off the coast of California, was more satisfactory and it was agreed that the aircraft would be deployed to the forward area on the 20th.

By then the array and the various responsibilities had been settled. Samuel would be in the control RC-121 with the appropriate Task Unit Commander, and Starbird and Ogle would be on an aircraft carrier in communication with Samuel and close to the target area.

In late September two more rehearsals for Androscoggin were held in the target area. On the first one the Sandia DME was questionable, the AFSWC DME gave nothing, and the LRL DME was quoted as being "no good." However, on the next dry run the system worked appreciably better, and, working around the high-altitude schedule, Androscoggin was finally fired on October 2. The Sandia and AFSWC DMEs worked properly,

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Among other people and organizations considering the safety of the Atlas systems test just before test resumption there was a "Special Ad Hoc Safety Group" who wrote a "Special Safety Study Report of the SM-65D (Atlas)/Mark III R/V," published in April 1962. Chaired by Air Force Colonel Edwin Miller of the Directorate of Nuclear Surety (other members included Bob Hilty of the AEC Albuquerque Office and Lee Hightower of Sandia), the group concluded this test could be conducted safely "provided that action is taken to reduce the probability of premature application of the prearm signals to the arming and fusing system." They also concluded that there might be RF interference between the General Electric Range Safety System (GERSYS) and the Mod 3 Guidance System. Furthermore, they recommended the alternative impact area to provide better protection for the Hawaii and Johnston areas. After making the brief recommendations to eliminate the noted problem areas, they seemed to be approving the conduct of the test.

Consideration of how to modify the system to allow carrying out the Atlas systems test, as well as to permit the scientific community of the Task Force to get the minimum data on the shot, continued through a fair part of the month of May. Roswell Gilpatric, Deputy Secretary of Defense, sent a memo on May 9 to the Chairman of the Joint Chiefs of Staff reporting decisions made by the President on May 8, including a reaffirmation of his disapproval of an Atlas operational test. Either the JCS did not communicate this fact to the operational force, or the Air Force didn't believe it was a final no, or they interpreted it as withholding approval until later. In any case, the Air Force still requested readiness preparations, and the test was pursued for some time before it was firmly canceled.

Ogle, as Task Force Scientific Deputy, tried to support the military systems tests by arranging for measurements ordinarily made by the various scientific Task Units, but the task was difficult because the military systems tests did not yield information of significant importance to these Task Units, e.g., weapons development or effects information. As a result he spent a fair amount of time during May trying to work out ways to obtain good yield, location, and performance data as well as coaxing the scientists to provide adequate support for the tests even though it was not their primary interest. On May 15 Dial Right was scheduled for June 6, if it was approved.

The Air Force sponsor for the test, Strategic Air Command, made a broad effort to meet the recommendations of those judging the safety of the test and to have the test approved. Their outlook was included in a message from SAC to the Air Force Chief of Staff on May 16. One safety feature that had been recommended was a device to prearm the warhead in flight. Such a device, consisting of an explosive switch and acceptable means for its activation, was engineered and was being installed in the three Dial Right missiles as of May 16. An installation similar to this had been successfully flight tested on May 11 "using the appropriate Dial Right prearm boundaries." Modifications to the General Electric ground guidance system to assist the PMR instantaneous impact predictor (IIP) plotting system had shown satisfactory coverage of the Dial Right target area, and the appropriate maps had been prepared. In addition, the Atlas system had been modified to include the General Electric Range Safety System (GERSYS) and this package had been successfully flight tested on both the Atlas and the Titan. Another improvement to the system was attempted as part of a calibration flight on May 11, but the performance of the system was erratic in its coordination with the PMR radars, and it was not considered satisfactory for the Dial Right test. The lack of time left to improve and check out an acceptable system led SAC to recommend that the Dial Right missiles be equipped with the prearm safety device and the GERSYS systems but not the C-band beacon. SAC felt that this configuration would satisfy the redundant IIP requirements and that the absence of the C-band beacon was not a safety hazard. Additional concern had been voiced about radioactive contamination of the Vandenberg local area resulting from accidental burning or single-point detonation of the warhead. A special safety study of these problems indicated that the effects of the worst possible accident could be minimized and contained within acceptable limits. SAC Headquarters had reviewed the disaster control plans in detail to ensure their adequacy. Based on all of these activities and their continual coordination with the Commander of JTF-8, SAC strongly urged final approval of the Dial Right systems test.

In spite of this effort the end came for this Air Force systems test on May 25. On that day Starbird was informed by the JCS that the test was canceled. He immediately expressed to SAC and the numerous other units associated with the test his regrets that the event could not be carried through and his appreciation to all of those involved in preparations.

There is a most important lesson here for the consideration of any potential systems test sponsor during any time period prior to nuclear testing. That is, no matter how important the execution of a national strategic or tactical systems test is considered to be to the so-called national defense or national security, the safety considerations to protect the people and property of the United States certainly carry a tremendous weight. Thus, the judgments, conclusions, and recommendations of those most familiar with the hazards must be taken seriously and met point by point, or repeats of the futile exercise carried out by the Air Force and SAC with the Atlas systems test will be experienced.

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Frigate Bird (Polaris/Navy)

On the day after the U.S. resumed atmospheric testing, Ogle distributed the tentative shot list which showed two approved DOD systems tests. The first of these, a test of the Navy Polaris missile [redacted] was scheduled for May 5. The target danger area, a 240- by 120-nautical-mile box added to the northeast corner of the Christmas Island danger area, was in effect from April 30 through May 10. In line with the projected schedule and previous decision that the JFT-8 Navy Deputy Commander would also serve as Commander of the Task Group for the Polaris systems test, Rear Admiral Lloyd Mustin left Christmas Island on April 28 to serve as Commander of Task Group 8.8 in execution of the Polaris systems test. The missile was to be launched in an operational mode from a submerged submarine, the U.S.S. Ethan Allen, commanded by Captain P. L. Lacy. For the Frigate Bird event two arrays of ships were involved, located 1,020 miles apart. The launch area array was composed of the Ethan Allen, a guided missile ship (AVM), a support carrier (CVS) with an embarked air group, and four destroyers. The purpose of all of this was to execute the launch properly and safely with precise ship positioning. In the impact area the array consisted of two submarines operating at periscope depth 25 miles from the impact point and positioned 45 degrees on each side of the flight path of the Polaris. Each submarine was equipped to provide scientific and documentary data, and, in addition, there was a small air array with diagnostic functions. The air array was composed of an RC-121 AOC, a C-130 diagnostic aircraft (the Livermore aircraft, number 299), a C-135 sampler controller, and B-57 D samplers. Admiral Mustin established the JTF-8 control point aboard the AVM, the U.S.S. Norton Sound. He was advised by Rear Admiral Levering Smith, Technical Director of the Navy Special Projects Office in Washington, whom he had requested be present for the systems test.

The test date was slipped from the planned date of May 5 to May 6 because of inability to maintain required communications between the deployed launch array and the Commander of JTF-8 at Christmas Island. This problem was overcome by various measures, principally through CINCPACFLT, who exercised control of frequency usage by assigning a series of frequencies exclusively to this function and silencing all other users of these frequencies throughout the Pacific. Special safety features aboard the Polaris missiles included range safety tracking beacons, destruct systems, and a separate battery power source. Additional safety measures required that the destruct safe-arm switches be moved to the arm position in a prelaunch sequence before the first stage missile ignition system could be enabled. Many rehearsals of the launch sequence were conducted, simulating the beacon aboard the missile by flying a beacon aboard some aircraft. The Ethan Allen crew could acquire the C-band beacons within six seconds after a simulated launch, giving them confidence that their radar would be locked on the missile soon after it broached the surface and before it reached the expected altitude of the usual low clouds. (If lock-on were not achieved the missile would be destroyed by range safety.) The Ethan Allen arrived in the launch area on May 2. Rehearsals on May 3 and 4 uncovered long-distance communication problems which led to a delay until May 6.

On launch day the weather in the launch and impact areas was predicted to be marginal but worth trying. Holds due to adverse weather conditions in the impact area caused delays of about two hours on the morning of the test. These holds came after switching the missile safety systems to internal power (beginning the drain on the internal batteries). At long last the countdown proceeded, but at 30 seconds before launch of the primary missile the fire order to that tube was automatically bypassed by the control system and the backup missile was selected, and then that tube was also automatically bypassed. Analysis quickly showed that the first bypass happened because the "muzzle hatch open" limit switch failed to close and the second

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bypass was caused by a false "safe/ready" indication. Only a few minutes were needed to correct the situation, after which the standard firing crew procedures were used to achieve proper indications. However, by this time Admiral Mustin thought the delay in the detonation time might be a safety problem for the observation aircraft in the burst area, and he ordered a range-safety hold in order to choose a new burst time. In the Task Group report on the test, the following was stated about the missile bypasses:

> It should be emphasized here that these missiles would have been fired in a true tactical situation, since there would have been time to analyze and correct these casualties with very little delay in a tactical countdown, where "burst time" is not such a restrictive consideration.

Thus, a new detonation time was established which still would be within the useful operating time of the activated internal batteries. However, just before the new launch time Admiral Mustin ordered another hold for launch area weather conditions. This hold led to the further complication of replacing the beacon and destruct batteries in the two missiles previously counted down, since their remaining operating times were now too short. During the several minutes required to effect the battery changes, the local weather reconnaissance aircraft indicated a favorable cloud situation approaching, and a new missile tube was selected, simultaneously cutting short the battery exchange procedure and precluding the possibility of utilizing either of the two original missiles. Finally, this third missile was successfully launched 18.5 seconds after the nominal launch time, the delay being "associated with the relatively slow hydraulic pressure buildup shown in the history of this missile and experienced in the final seconds of its terminal count." Following launch the range safety observations showed a nominal trajectory and nominal time of flight. Diagnostics were limited, data coming from three sources. The two submerged submarines in the burst area observed the burst with bhangmeters mounted to see through their periscopes. Secondly, the B-57 D sampler aircraft controlled by the KC-135 (all under Task Group 8.4) successfully sampled the burst cloud at a location about 525 nautical miles (near their maximum range) from their Christmas Island base. Thirdly, the Livermore C-130 diagnostic aircraft positioned by the RC-121 attempted to acquire diagnostic data from the test, but this attempt was essentially fruitless, primarily because of inappropriate positioning in this all-airborne configuration.

Rough estimates of the burst height and location based on the observations in the impact area indicated that the burst was at about 8,300 feet altitude and 1.25 miles from the nominal aim point. [redacted]

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The report on the Frigate Bird test from the Commander of JTF-8 to AEC Headquarters stated the following:

> Three holds were imposed because of weather and one for technical reasons prior to launch . . . one tube experienced a failure of the launch hatch interlock and another tube lost the 800-cycle conference voltage during a simultaneous countdown of two missiles. Countdown continued on Tube No. 9. A slow hydraulic system buildup indicated a no-go condition that required recycling. This accounted for the additional 20 seconds on launch.

An illustration of "filtering" of information to minimize what might be considered failures is found in a letter from Chairman Seaborg to President Kennedy on May 18, which discussed the progress of the test series. As for the systems tests, Seaborg simply reported that they both "functioned reliably on the first firing."

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Swordfish (ASROC/Navy)

This test of the Navy's antisubmarine rocket system was, as mentioned before, both a system test and an underwater nuclear weapons effects test. Just as for the Polaris test, a separate Task Group, TG 8.9, was temporarily established within JTF-8 to execute the Swordfish event. By May 3 two full dress-rehearsals had been conducted in the San Diego area, with full participation except for the carrier surveillance group that was engaged in the Polaris test at the time. One ASROC weapon was fired by each of two destroyers in a nonnuclear mode during these rehearsals. One of these destroyers, the U.S.S. Agerholm, was the primary firing ship on the Swordfish test, and the other destroyer, the U.S.S. Anderson, was the standby firing ship.

The units supporting the test in the San Diego area departed for their stations beginning on May 4, the majority departing on May 7. After the Frigate Bird Polaris test on May 6 Admiral Mustin sailed aboard the carrier Yorktown to the Swordfish test area. Following the May 8 authorization to perform the test, Mustin and the Yorktown reached the array of all other Swordfish units in their assigned area on May 9, and Admiral Mustin shifted to the LSD Monticello, which was his flagship for the test. The ship array to conduct and observe the test consisted of three destroyers, a submarine, the landing ship dock (LSD) on which the Task Group Commander established his command center, and the support aircraft carrier. As on the Polaris test, the overall command was from the JTF-8 Commander at Christmas Island, but it was exercised through the TG 8.9 Commander in the ASROC area.

The towed instrumentation array, which included the target raft, the unmanned destroyer to measure effects (U.S.S. Bausell), and ten instrumental platforms, was over six miles long. The numerous pieces of instrumentation in this towed array were all assembled as one string on a polypropylene tow line. The target raft carried a radar target for the radar systems on the launch ship and a sonar reflector suspended underneath to simulate a submarine target for the sonar systems of the launch ships. The instrumented array was towed into place as planned by an ocean-going tug, the rest of the ships took their assigned positions, and the countdown commenced early on the morning of May 10 in anticipation of a noon event time. However, as noon approached, low cumulus clouds in the burst area threatened to obscure the view of the A3D aircraft responsible for critical photography from above the burst. The photo aircraft were moved into a new pattern in the hope of achieving zero time coincident with holes in the clouds. What transpired is best related by quoting from the Task Group report:

> During this process, there then occurred a succession of further interruptions from individually minor items. First, the lead A3D had an electrical power failure. The standby plane was slow in the planned takeover, mainly because of the considerable communication confusion which attended this casualty. Then Agerholm momentarily lost power on the switchboard providing primary power to the ASROC system, through faulty procedure in the engineering department. This lost the "green board" system indication on the bridge, which was misinterpreted to indicate a masked battery, and led to clearing maneuvers which put the ship in poor position relative to the planned test firing conditions. Finally came a report (later found erroneous) that the start-camera had been sent on a nonfiring run, which would have disabled critical technical photography for the actual run. At this point, with the A3Ds nearing the end of endurance on station and cloud cover progressively worsening, the event was canceled for the day and rescheduled to May 11.

The initial feeling was that achieving readiness for May 11 would be an impossible task because of necessary reorientation of the instrumentation array and other coordinated requirements, but owing to "magnificent seamanship with the array and extraordinary efforts by the instrumentation technical groups," all was made ready for a repeat attempt one day later. The ASROC test was conducted successfully on May

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11 at 1 p.m. The time of flight of the ASROC was about 40 seconds from the Agerholm to the burst point about 4,000 yards down range, and the actual burst occurred about 350 yards beyond the nominal aim point. Measurements indicated that the burst depth was [redacted]

The test yielded a wealth of effects information, but only a small part of the planned system test diagnostic data were recorded. Included in the data gathered were information on the base surge, the water contamination, effects on ship sonar, damage to a variety of ships in various positions, and off-site hydroacoustic effects. Some of the data gathered resulted in significant recommendations on tactical employment range concepts and on the need to investigate the premature detonation probabilities of the ASROC fuse.

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The High-Altitude Series

The President directed the AEC on April 24, 1962, to resume testing, just two days before the first rehearsal for Tiger Fish, which was to be the certification launch of the Thor missile from Johnston Island. As with any good rehearsal, the intention was to have a complete array of aircraft and ships and to have all experimenters on the dry run, but not to fire the small rockets, or at least not very many of them. The LASL C-135 aircraft did participate in the dry run, as did most of the air array and all of the ships. However, the DASA aircraft were not yet ready, needing those last few days to prepare.

Trouble with the pod orientation continued, and the certification shot which would carry pods was delayed until May 2. On April 25 or 26 Douglas and SSD concluded from analytical and wind tunnel data that they could reconfigure the ballast in the Mark 5 RVs to provide the capability of carrying two RVs and one pod on Starfish. Additional wind restrictions for the launch were involved, but they did not seem to be particularly difficult.

Tiger Fish was fired on the morning of May 2. The missile itself and all the warhead certification features operated properly. Range tracking was satisfactory. The pods were recovered satisfactorily. Unfortunately, the flywheel drive motors used to establish pod orientation burned out, but one pod had one flywheel running slowly at launch time. As a result, two of the pods tumbled almost immediately after release from the missile and the third showed a 20 percent wobble at what would have been burst time. The long-range communication was unsatisfactory and some "go, no-go" information was not relayed to Johnston in time to be acted upon.*

Between Tiger Fish and the first attempt at Bluegill, long-range communication was improved and a proper size motor was put on the pods. Several dry runs were conducted between the two shots, as were a couple of air array rehearsals.

Shortly before Bluegill Norman Thomas, in a telegram to Seaborg, suggested:

> In view of opinions of British scientists and others, is there not grave danger in sudden unilateral American decision to explode three "rainbow" bombs possibly affecting seriously the Van Allen belt? Is not the mere fact of these protests sufficient reason for indefinite delay at least until international consultation?

The problem was to grow during the operation.

*The center of the communications net in Honolulu lost voice and teletype transmission from Johnston Island shortly before launch. They continued, however, to transmit "blind" to Johnston, and most of the critical information was received at Johnston, including the status of Haleakala, Kauai, Point Arguello, etc.

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The AEC and DOD Task Units had established joint technical operation centers at both Johnston Island and Hawaii. These operation centers coordinated their actions more and more in the time between Tiger Fish and Bluegill in order to present to the Task Force Commander a moderately unified view of the situation. "Go, no-go" criteria established included such items as (a) excellent seeing conditions between the ground optical stations and the burst, (b) appropriate launch winds for the Thor and the small missiles, both from operating and safety points of view, (c) conditions of solar magnetic storms, (d) the requirement that at least two pods be stabilized and capable of recovery, (e) communications to the far-out stations being in operating order, and (f) an appropriate fraction of the observational aircraft operating. These criteria varied from shot to shot, depending upon the requirements for the particular shot.

While the remaining Bluegill experimenters were moving to the field, setting up their equipment, establishing communications, etc., and while Shuster, Ray, and others on Johnston Island were establishing the control system and agreeing on safety and other no-go conditions, etc., the AEC and DOD in Washington continued their pressure on the President to add tests. At the May 8 NSC meeting the President agreed to include Urraca in the series. However, the system could not leave the program alone. A query concerning the effects of a high-altitude nuclear detonation on the Van Allen belt and the possibility of satellite damage reached high levels very quickly. In response to a question from Colonel Anderson of DMA, Conrad Longmire stated:

> It is expected that the high nuclear shots, Urraca and Starfish, will have some small, but possibly measurable, effects in the region of the inner Van Allen belt. The effects fall into the following two types: (1) injection of energetic bomb-produced electrons and protons into the belt .... In summary, it is my strong belief that the perturbations produced on the inner belt will be minor, if detectable at all. Furthermore, if detectable perturbations are produced, a measurement of the relaxation time of the perturbation would add a positive contribution to our scientific understanding of the belt.

Three days later Longmire and Taschek expanded the point for Commissioner Haworth. [redacted] They noted that the measurements would be made mostly from rockets launched from Kauai and Vandenberg Air Force Base and that there would be extensive photography coverage. They again commented that the perturbations to the inner Van Allen belt were expected to be small. On May 15 Seaborg briefed PSAC on the problem. After thinking about it, J. Wiesner (the President's Scientific Advisor) inquired whether the yield of the device could be reduced and still allow a good experiment. [redacted] 24 Seaborg, as previously requested, reviewed the discussions of the last few weeks for the President. After again giving all of the reasons for its inclusion and the

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reasons for not changing the yield, and further stating that it was too late to modify the booster to utilize a lower-yield warhead, he concluded that another look would be taken on the subject of reducing the yield.

On May 24 Seaborg, Haworth, Harold Brown, J. Wiesner, McGeorge Bundy, Carl Kaysen (Bundy's Deputy), Spurgeon Keeny, and General Betts met to address the problem. Wiesner did not believe that a demonstration was necessary in order to convince ourselves that we could carry out deep space tests. Seaborg took a firm position that there would be definite technical value in carrying out the experiment [redacted] in the proper medium. It was concluded that there was no eyeburn hazard. The group finally decided to leave the shot in the program, apparently because of the argument (presented by Bundy) that the President probably would not wish to be accused of giving in to British scientific pressure.

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Preparations for Bluegill proceeded. On May 8 Gilpatric notified Betts of his intention to appoint a Joint Board of Investigation which could be convened immediately by the Commander of JTF-8 in the event of a major nuclear accident or incident involving loss of life, damage to public property, or serious public reaction. The Board membership would consist of two representatives from the AEC and a Field Grade officer from each service and DASA, with retired Vice Admiral E. M. Parker as chairman.

DASA began to move on the McMillan Committee suggestions for additional shots. On May 11 they proposed "Red Snapper," [redacted] at 20 kilometers altitude. Associated with the burst would be powered pods positioned above and below the device during reentry to observe the effects on the pods. DASA also reentered Kingfish into the plan in mid-May.

The addition of these shots would only be sensible if more Thor boosters could be obtained. On May 16 the Director of Materiel Management at Norton Air Force base (SBAMA) explained to the Chief of Staff of the Air Force:

> SBAMA literally scraped the bottom of the barrel in providing a total of four SM-75 (Thor) missiles for Project Fishbowl launch operations. The two additional SM-75 missiles provided Fishbowl are emergency backup missiles which were assigned to Fishbowl with a definite and agreed understanding that they would be returned to SBAMA at the conclusion of Fishbowl to meet commitments to USAF/RAF operation program in accordance with existing country-to-country agreement signed by U.S. and U.K. governments. As a consequence, the emergency backup missiles are not surplus which are available for other projects at the conclusion of Fishbowl operations.

They also noted that one missile might be obtained without involving the U.K. by using a missile allocated to SAC at Vandenberg for display purposes. That missile (No. 150) was then being used by Douglas to assist in engineering testing for Fishbowl. They recommended immediate authorization for rework of missile No. 150, to be used either for Red Snapper or Kingfish, and urged that only one of the two projects be done. SSD commented that Kingfish could be no sooner than July 21-24 and Red Snapper no earlier than August 15-18. By May 25 Kingfish was approved for planning as the last shot in the series: presumably it would be fired only if appropriate materiel and time were left for it.

On May 11 the Commission approved Starfish and Bluegill for execution. Schlesinger wrote,* concerning the effort to build the international machinery of peace in mid-May 1962:

*Schlesinger, *A Thousand Days*, page 503.

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> The President, therefore, maintained a steady pressure on the executive branch to keep the negotiating effort alive. Wiesner and Kaysen flourished the White House mandate, were tireless in needling the bureaucracy and forcing disarmament issues; and Bundy intervened valuably at critical moments. Wiesner often carried the brunt of the argument against the Pentagon in meetings before the President. After one contentious session, he told me that he was afraid that he had talked too much and might be wearing out Kennedy's patience. Later the President asked me about the meeting. I said it had filled me with gloom, that only Wiesner had made much sense, and that he was afraid he had done more than his share of speaking. Kennedy smiled and said, "Sometimes, I think Jerry talks too much, but I didn't think so yesterday. Tell him that I thought he made a series of excellent points and that I want him to keep it up." Next to the President, McNamara, with the able backing of John McNaughton probably did more than anyone else to sustain the disarmament drive. With his sense of the horror of nuclear conflict, his understanding of the adequacy of existing stockpiles, his fear of nuclear proliferation, his analytic command of the weapons problem, and his managerial instinct to do something about an irrational situation, he forever sought new ways of controlling the arms race. His contribution was especially crucial in dealing with the Joint Chiefs of Staff, possessed as they were by the conviction that they alone understood the requirements of American safety. Nor was the invocation of national security confined to the JCS. Once, at a meeting of the Committee of Principals, someone from ACDA objected to a proposed arms control measure on the ground that it might imperil the nation. McNamara said sharply, "If I'm not afraid of it, I don't see why you should be. You take care of disarmament. Let me worry about the national security of the United States."

On May 20 the Chief of DASA concurred with Kiley on the recommendations for two RVs and one pod on Starfish. Kiley would keep the capability of using three pods in case new problems came up.

By mid-May LASL had loaded up their C-135 with so much instrumentation that when fueled for the rather long-range mission they planned, the weight of the plane approached the maximum operating capability utilized by SAC. The result of discussions between the scientists and the Air Force was a clear recognition and understanding by both SAC and LASL that aircraft safety was the responsibility of the Air Force. While there were occasionally some tense situations on takeoff from Hickam, flights operated as planned and Air Force judgment was validated.

The problem of possible eyeburn in Hawaii was raised by introduction of Kingfish, which was planned to be [redacted] altitude over Johnston Island. The initial calculations indicated that a danger area 600 miles in radius would be required at sea level and a greater radius at aircraft altitudes.

Late in May, as the date for Bluegill approached, other operational decisions were made. Betts issued a statement to all Labs that there would be no message traffic from Johnston Island to those organizations in the period from 24 hours before to 24 hours after Bluegill because of the heavy communications traffic needed for operational reasons. In the last two weeks of May, under Shuster and Ray, several Bluegill dry runs were conducted in a very realistic fashion. There were communication troubles, aborts because of weather, aircraft trouble, etc. The Johnston Island system was doing a fine job of making the rehearsals as realistic as they could. In the middle of all this, Salet got his second star, but the party that night at the Point House didn't slow down the operation very much. Starbird and Ogle moved to Johnston Island a few days before the Bluegill event, which was scheduled for June 2.

Bluegill was counted down for the first time on the night of June 2, 1962. At minus 45 minutes the Range Tracker (the PMR range safety ship) computer failed and there was a two-hour hold, but the test was then cancelled for that day. The cancellation arose from the requirement to know the burst-time position of the RV to within two miles because of the positioning of effects ships. The Range Tracker computer was essential because it computed predicted positions from data on the missile position acquired from launch through main engine cut-off time. PMR worked desperately all day on June 3 to repair the computer, managing to get some computer experts from

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Los Angeles to the island at 3 a.m. on the 4th, just after PMR got the computer fixed. Bradbury was on the same 3 a.m. flight[redacted]

The first launch of the Bluegill Thor [redacted] occurred just after midnight on June 4. Earlier in the evening the weather had been cloudy, but Dan Rex had predicted it would clear and it did by about midnight. After a few holds for minor reasons the Thor missile was launched, apparently achieving a perfect trajectory. However, in the last stages of propulsion, just before main engine cutoff, PMR lost track from the beacon-tracking radar used to calculate the predicted burst position. There was some chance that a second radar, which was skin-tracking the missile, might have acquired sufficient data to validate the impact prediction. Therefore, attempts were made between about minus 10 and minus 5 minutes to use the skin-track data for burst-point prediction, while also attempting to reacquire the beacon, but neither was successful. Consequently, at minus 5 minutes Ogle suggested, and Starbird agreed, that the warhead should be destroyed, and that was done.

In a discussion of predicted impact computations following the end of the test the path of the predicted impact point appeared normal through main engine cutoff. However, during the vernier engine phase the predicted impact position shown on the plotboards seemed to change excessively, based on the beacon-tracking FPS-16 radar. But data from the skin-tracking MPS-26 radar indicated a smooth and nominal path. Which radar, if either, was telling the truth is, unfortunately, unknown.

Years later Frank Strabala of EG&G pointed out to Ogle that the timing system run by EG&G, which provided signals to all of the cameras and other missiles, etc., had failed appreciably earlier in the flight. However, EG&G had seen no point in bringing up the subject since the flight already had troubles. In addition to the problems already mentioned, telemetry data discussed in the postmortem showed that two of the three pods failed to separate. The JTF-8 control room voice tape record shows that a few moments after "destruct," as the pieces were falling down, Ogle commented, "Best damn dry run we ever had."

On the Honolulu beaches the many people who had turned out to watch the flash in the sky were disappointed. The next morning, the Honolulu Star Bulletin quoted a JTF-8 spokesman: "There was no nuclear detonation and there is no likelihood that the fragments of the device will explode in the ocean. Nor is it considered that it will cause hazardous levels of radioactivity in the water, and they will not constitute a hazard to human health."

An appreciable number of the small rockets (and the equipment they were to carry) had been saved on Bluegill, either because they had been cancelled before the Thor launch because of improper winds or because the launch countdown timer had been stopped before they were launched. There was a spare Bluegill RV and nuclear explosive device on Johnston Island. On the other hand, the pod-orienting system had to be rebuilt. The Starfish shot had all of its parts coming down the pipeline and had been planned as the next shot. Furthermore, Starfish was a somewhat more important shot. Thus, for these and various other reasons, the decision was to do Starfish next.

However, something had to be done about the tracking systems. The Range Tracker radars and computer were the first problem. After some discussions it was arranged that the two radars would operate separately, one on beacon track and one on skin track. Each radar would feed its information into the computer memory separately. Because of limitations on the computer, only one set of data would be used in a time to compute the refined impact point, but it was arranged that if that tracking system failed, then the computer could use the data from the other radar track, recomputing the track in about three minutes.

The Range Tracker also had a real-time presentation of the missile horizontal range versus altitude, obtained from its primary radar. Since the intended trajectory was known, these data were sufficient for safety purposes if they were available after main engine cutoff. However, it took a trained man watching that data to derive that information. PMR had such a trained man, and it was arranged that he would watch that data specifically, advising the Task Force Command Post 200 seconds after lift-off whether everything was all right or not (via a specially arranged hot line).

Field Command had arranged for the Cubic Corporation to be responsible for tracking all of the DASA instrument packages, including one of the pods. Cubic Corp. used a very accurate microwave interferometer system known as angle measuring equipment, distance measuring equipment, or AME/DME. On the first attempt at Bluegill the AME/DME data had not been presented in real time. Arrangements were quickly made to use it to get a nearly real-time plot of the surface range and azimuth, which, if compared with predicted values, could also be used for safety purposes.

Sandia operated a similar system using the transponder on one of the RVs, but this system would give only a slant range in real time. It was therefore arranged to have a real-time presentation of the slant range that could be compared with the record from Tiger Fish, since the two trajectories were supposed to be identical. Lastly, even after the missile range exceeded the Range Tracker capability, that radar could give the angular elevation of the RV to within about 2°. In addition, the Cubic Corporation microwave interferometer could give the angles to within about 1°. The combination of these angles and the slant range would give an approximate position, which then could be compared with the expected position at a given time. The proper arithmetic had to be done by hand calculation, and a team of people in the control room were given the job. The prime calculators were Dan Rex and Vay Shelton. They could give the position about one minute later than real-time.

It took about two weeks to institute all the new tracking procedures, and then several days were used feeding in data from old tapes (Tiger Fish), etc., so that by June 19 there were a number of independent systems which would give RV position within a minute or two after it passed through a given point.

On June 7 JTF-8 planned to do Starfish on June 18 and Urraca two weeks after that (approximately July 3), with a repeat of Bluegill not scheduled precisely. The problem in scheduling a repeat of Bluegill was not only the mechanical one of rockets and pods, but the political one, since the President had indicated the series should be done by the end of June. However, the next day, the JCS sent a message to Starbird and Bradbury stating their desire that Bluegill Prime, the second try of Bluegill, be fired during the current series and giving their opinion that the Urraca shot "must not interfere in any way." The JCS suggested that the next shot should be Starfish, then Bluegill Prime, and after that could come either Urraca or Kingfish if Urraca should not be fired. Harold Brown and Gerry Johnson concurred with this opinion. However, it soon became clear that all of the equipment for a repeat of Bluegill could not be constructed soon enough to do Bluegill two weeks after Starfish, and, therefore, on June 15 the Task Force reiterated the schedule, according to which Starfish would be done about June 19, with Urraca as soon as possible after that, but not earlier than July 3. Bluegill would be attempted after Urraca. However, the Washington debate persisted, and Betts asked Starbird to consider other schedules that would include Kingfish. Knowing the President's desire to keep the operation as short as possible and recognizing the weight that could be exerted by the Department of Defense, Hoerlin promptly asked Taschek to go to Washington and join the argument in defense of Urraca. Hoerlin commented to Haworth on the strong need for the AEC scientific side to get the data in the regime which Urraca would investigate and noted with respect to the proposed Kingfish shot that "My colleagues and I would like to state that after a successful Starfish and in view of the

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similarity with Teak, it should pose no problem to compute both the phenomenology and the effects of such an event."

The picture was clouded even further on June 19 with the first attempt at Starfish [redacted] For that shot, the missile carried one pod and two RVs externally. The weather was acceptable and the Thor was launched a little before 11 p.m. in order to have an 11 p.m. burst time. However, 59 seconds after launch the missile flared and exploded. The warhead destruct signal was sent 64 seconds after launch. Debris rained on the island, fortunately doing no serious damage. Investigation of the pieces and the telemetry data made it clear very soon that the mock RVs carried up with the missile had disturbed the flow of gases from the turbine exhaust, sucking the hot gas back against the boat tail and weakening the structure. The engine had torn itself loose and flown right through the fuel tanks.

In the meantime, Hoerlin, Longmire, Ogle, etc., had been considering the eyeburn problem in the Hawaiian Islands presented by the proposed Kingfish shot [redacted] That same problem had led to an odd-shaped danger area for Starfish, circular at sea level and at aircraft altitudes, but increasing in radius with altitude. Since this danger area would require rerouting of commercial air flights, the Commission requested information on the subject. The information was provided by Hoerlin and Ogle, who explained the problem to the Commission during a meeting on June 7. The Commission agreed with the proposed danger area, as did Jerry Wiesner later in the day. Samuel, Mustin, and Ogle visited President Kennedy in the afternoon to brief him on the operation. The President expressed a desire to hurry the tests (Ogle said we could not) and got one more view on the reasons for the high-altitude shots.

However, according to calculations, Kingfish would present an eyeburn problem in some parts of Hawaii. Even though the shot might be fired late at night or early in the morning, experience had already shown that there would be a number of Hawaiians in the mountains trying to observe the detonation.

The scheduling problem became very difficult. In the first half of June DASA and JCS reviewed the priority of the DOD shots and decided that Starfish was their most important test. Field Command DASA stated that there were sufficient pods, including ones that were being refurbished, to provide three pods each for another attempt at Starfish and Bluegill and three additional pods in the event Starfish needed to be repeated a second time. They concluded that there should be no further attempt to fly RVs, and Douglas concurred. Thus, from their June 20 meeting the JCS concluded that (a) the next shots should be Starfish and Bluegill and (b) Urraca after those was uncertain depending upon the situation at that time. Betts transmitted that information to the Laboratories and the Task Force, stating that he had sufficient information on Urraca and did not need any further arguments. (On the 19th Hoerlin had reiterated in a very strong message all of the reasons for doing Urraca and the reason for the AEC insisting on the shot, pointing out the tremendous effort that Los Alamos and Sandia had put into the shot so far.)

However, the argument did not stop. On the 21st Schwartz of Sandia echoed Hoerlin's sentiments to Haworth and Betts, and Ogle was in Washington to explain the operational problems again at high levels. The argument did result in the cancellation of Kingfish, but the order of shots was not Starfish, Bluegill, and Urraca, as the President had agreed to on June 20.

President Kennedy, upset at the series of failures, asked the Department of Defense why the Thor had been chosen rather than some other missile such as Redstone. Gerry Johnson reviewed the reasons for the missiles (see missile selection section earlier), pointed out that there were three Thors left at Vandenberg, with one scheduled to be shipped within the next two days, and added that the Air Force had

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seven additional boosters which could be configured in a minimum of two months. [redacted] Accordingly, the next attempt was to be Starfish, and rumors circulated that July 9 would be the end of the atmospheric testing. LASL continued to argue for Urraca right after Starfish, claiming that it was more important than Bluegill. On June 24 Starbird requested that another Starfish RV and warhead be delivered by July 9 in case the proposed July 4 Starfish repeat should also fail.

On June 25 Hoerlin sent a message to Wiesner, Starbird, Ogle, Johnson, et al. concerning Urraca. In addition to previous arguments, he debated what he called the principle of noninterference, which was interpreted as meaning that Urraca must wait until the DOD was absolutely finished with its program, regardless of how many repeat shots were necessary. He did not see how the AEC could agree to do its work only when it did not interfere with the DOD's schedules without being derelict in the duty assigned to it by law. However, he went on to argue the military importance of Urraca, commenting that each of the three shots (Starfish, Bluegill, and Urraca) would occur in completely different atmospheric regimes and that our lack of understanding of the dominant mechanisms controlling the explosions was such that any of the shots would produce important information, but that Starfish seemed most likely to do so. On the other hand, he argued that Bluegill phenomena would be most closely related to low-altitude detonation phenomena, and it was, therefore, more likely to be predictable by calculations. Consequently, the order of firing should be Starfish, Urraca, Bluegill. He also brought up the point that Urraca was important as a test of our space detection system, and the lessons to be learned could prevent the Russians stealing another march on us by preparing to test in deep space.

The McMillan Committee met in Hawaii on July 13 en route to observe the Starfish Prime shot, again re-examining the priority of the three remaining high-altitude shots. They found no reason to alter the previously recommended order of priority, recommending that Starfish be tried until it was successful, followed by Bluegill until it was successful, and then, finally, to do Urraca. However, they did comment:

> The effects associated with Urraca have sufficient interest and potential to merit DOD support. Weapons effects cannot be reliably extrapolated in the new altitude regimes. The Urraca could contribute significantly to the understanding of high-altitude effects and thus, indirectly, answer many questions now in a speculative stage.

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At the beginning of July McGeorge Bundy reviewed the problem [redacted] Urraca were included it could not be carried out before August 3 and might slip as much as a week due to weather requirements. Against the test was the argument that there was no serious military interest that far out into space, that neither space testing nor space test detection were of current urgency, and that the United States was running overtime on high-altitude tests and should stop before August. On the other hand, Bundy commented that Urraca was the most interesting shot in strictly scientific terms as compared to Starfish and Bluegill, and that knowledge about space testing and test detection in outer space would make it more practicable to propose an atmospheric test ban. He pointed out the great amount of effort that had gone into the Urraca shot so far and noted the morale difficulty, stating:

> But it will not be easy for them to understand why one third of their eight months of effort should be rubbed out for a gain of two weeks in the end point of the series.

Finally he stated:

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Technically, Urraca is probably the most failureproof of the three high-altitude space shots. Its missile does not have the special gear of Starfish, and it does not have to follow the close track of Bluegill. It is in the hands of the Los Alamos Scientific Laboratory, which is, on the whole, the most experienced and most reliable testing agency we have. (It is one of the most abrasive aspects of possible cancellation that it would appear to penalize Los Alamos for the failure of its friendly rivals in Defense.)

On July 4 Starfish Prime was ready. The early steps of the experimental preparation had begun. The area had been swept. The FAA had been notified and the first aircraft were leaving Hawaii. However, at minus three hours, Douglas and SSD informed Starbird that the wind shear was too high, and the launch was canceled. In later discussion between the Douglas people on Johnston and those at home, new wind shear limits were adopted which would have allowed the launch early on July 5. The next few nights were unacceptable because of clouds. On the night of the 8th the weather was not perfect, but it was good enough. After the earlier failures, the test organization was nervous. Starbird wouldn't watch the TV pictures of the missile. Starfish Prime was launched on July 8. The Thor flew properly, the small rockets worked properly, and tracking went properly. In fact, essentially everything worked properly. The nose cones from the Kauai rockets were recovered. The pods were recovered. There were no eyeburns. The only problems were some cloud cover at Tonga, one pod flywheel that apparently ran slow and allowed the pod to tumble, and two instrument rockets from Point Arguello that failed. The shot was an outstanding success.

While the results of Starfish Prime would be subject to study for many years after the shot, some points were known fairly quickly. [redacted]

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The AFSWC people were especially happy with the results, as is shown in a message to their Headquarters: "Success of experiments is extremely gratifying to AFSWC personnel who have pointed space physics research to this achievement for several years. Suggest you pass congratulations to Research and Test Directorates and other AFSWC nuclear testing programs." Hoerlin was also happy, but in reporting the success to Bradbury at LASL, he noted the failure of the Arguello rockets and commented that the motives for conducting Urraca had been strengthened rather than weakened.

The successful firing of Starfish Prime therefore left two shots to be fired and two missiles available. Curtis LeMay, then Air Force Chief of Staff, noted in mid-July to Gerry Johnson that the Air Force was preparing an additional Thor in case the next Bluegill should fail. The missile was to come from those committed to the United Kingdom Air Force Training Launch Program. He also stated, "In view of the above, it is recommended that the additional Thor missile now being readied for shipment not be considered for any use other than backup for those high-priority DOD tests currently scheduled." (The fact that LeMay talked of "tests" probably reflects the Air Force's continued interest in Kingfish.)

Bluegill Prime was scheduled for July 25. LASL reported to Betts that the results of Starfish "go a long way toward proving out the feasibility of getting good bomb diagnostics from instrumented rockets in a space testing program." It appeared

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that the worst problems had been solved. On July 23 things looked smooth enough that the Task Force planned to fire Urraca 11 days after Bluegill, assuming Washington agreed. However, on that same day Ogle noted to Luedecke, "I've become aware that there is still consideration in Washington of proposed [redacted] Kingfish as a possible detonation of Operation Dominic after Urraca." He went on to note that the proposed shot would be quite bright and would produce an image on the retina of the human eye at the distance of Hawaii, roughly two thirds the size of the sun's image. He pointed out that for such a shot Kauai would possibly be within the danger area and that, certainly, people in the mountains at Kauai would have to wear dark glasses or turn away. Precautionary actions would have to be taken on Nihoa and Niihau. Small boating and fishing within the area would have to be stopped during the hazardous period. He noted that this situation could be improved by changing the missile trajectory to fire Kingfish some 200 to 250 miles south of Johnston, but that it would take two months after notification to change the Thor trajectory in that way. He also pointed out that lowering the burst altitude could solve the problem.

The next day SSD was busy calculating the additional costs of a seventh Thor to be used for the tentative Kingfish event and discussing the question of which kinds of pods could go on the missiles.

However, the picture changed rapidly. On July 24 the Bluegill Prime launch was delayed by weather, but the decision was made to go ahead on the night of the 25th. Shortly after 11 p.m. the Thor launch was attempted.

The missile ignited 20 to 30 seconds early and was burning in the boat tail and around the missile before lift-off time. The range safety officer therefore destroyed the missile and warhead within a few seconds after receipt of the lift-off signal, in order to prevent a large fuel explosion on the pad. In fact the Thor rose just a fraction of an inch and then settled back on the pad and began to burn. The fuel continued to burn for some time; at midnight personnel had still not been allowed outside because of the possibility of nearby fuel tank explosions, but by 12:30 a.m. it seemed safe. Inspection showed that the launch pad was badly damaged and was seriously contaminated with plutonium.

At 5 a.m. on July 26 JTF-8 issued the following statement in Hawaii:

> **A check with Johnston Island discloses no injuries to personnel and no hazard from any radioactivity as a result of the deliberate destruction and burning of a Thor booster and nuclear device on the launch pad last night. All missile fires have been extinguished.**

Assessment of the damage and the situation for future shots started immediately. [redacted]

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A very early estimate of the repair time was approximately one month, but after discussions with Douglas and SSD personnel it was apparent that it would be appreciably longer than one month. While estimates were being refined Starbird and Ogle left for Washington to promulgate further decisions. On the way through Hawaii Ogle requested that LASL prepare another [redacted]

The controlling factor in preparing for another high-altitude shot seemed to be the Thor pad. By July 28 it was fairly clear that an extremely optimistic schedule for repairing the present pad would have the pad ready in approximately eight weeks. Some two weeks of this was for cleaning and decontaminating the pad, about three weeks would be needed for procuring, inspecting, shipping, and installing all replacement items, and check-out of the rebuilt pad and support equipment would take another three weeks. By the time Starbird reached Washington, SSD and Douglas had determined that, given the proper priority, a second pad could be obtained in 13 to 15 weeks. Many other items were also critical, not the least of which was scheduling the USAS American Mariner (DAMP ship) which was necessary for some of the radar measurements.

Although rumors were rampant that the President would call an end to the operation, this was really very improbable. On July 22, 1962, the U.S.S.R. had announced their intention to begin a new series of atmospheric tests. Kennedy was still trying to pressure the Russians about a test ban. Furthermore, in the game of international strength it was bad enough for the U.S. to have such publicly miserable failures; it would be even worse to stop the tests, admitting that we could not finish the job. Furthermore, both the AEC and the Department of Defense had solidified their reasons for the experiments yet to be done. Thus, within a day or so after Starbird reached Washington the decision clearly was to finish the series, but the President was still in a hurry and urged a "crash" effort.

Upon his arrival in Washington at the end of July, Rod Ray started investigating backup systems that might be available. While Ray searched, Starbird notified the system that it should proceed with the second Thor pad with the understanding that Thor work could be stopped if other systems appeared more reasonable. H&N received AEC authorization to begin the pad on August 1. In the midst of this flurry the DOD requested that Kingfish be put back into the schedule, so the search for new launch systems also included that shot. A first alternative seemed to be a Polaris fired from a submarine near Johnston Island, using the same command destruct system that had been used for Frigate Bird. Other possibilities were the Hercules, Pershing, and Redstone.

Two other problems arose late in July and early in August 1962. The McMillan Committee observed that there were three shots left, that is, Bluegill, Urraca, and Kingfish, and that the Thor turnaround time was two weeks. They therefore inquired through DDR&E whether it would be possible to insert into the schedule a couple of fairly small high-altitude shots (with simple delivery systems, such as the Hercules) at two-week intervals, and the Task Force system promptly starting working on this. At the same time PMR decided that they could no longer afford to have the Range Tracker at Johnston Island and informed Starbird of their intent to take it back to California. The problem was solved rather quickly by Starbird and Mustin who dealt with the appropriate authorities in Washington: the Range Tracker stayed.

While the Johnston Island pad was being cleaned up during the first two weeks of August, a continual series of meetings and discussions gradually resolved the other carrier possibilities. It appeared that Sandia could put an RV with appropriate fusing and firing on a Polaris missile, and there was the possibility of launching either from a submarine or from the ship Observation Island. However, the Polaris allowed only a very short time for detonation decision after main engine burnout, and any hesitation would allow the possibility of the warhead getting to any of many inhabited areas. Furthermore, a certification shot would be required with such a system. This seemed to be a tremendous amount of work for a backup and it wasn't at all clear that the Polaris system would be any less trouble than the Thor. The Polaris proposal was eventually turned down. The equipment at Vandenberg AFB Thor Pad 8 could be moved to Johnston, but would require approximately eight weeks of construction and then six weeks of installation, at a minimum. However, this option also involved moving an appreciable number of experimental installations on Johnston Island, and finding a suitable place for all of those seemed very difficult. Furthermore, Douglas and SSD pointed out that if there were another accident on the first pad, it too could probably be cleaned up in about eight weeks; thus, putting in a second launch pad would not really save much time. The Army discussed their systems. The Pershing could reach the altitudes for either Bluegill or Kingfish,

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carrying nuclear devices [redacted] They preferred only their standard trajectory, but they could modify it with some degradation in confidence. Command arm, fire, and destruct signals could be provided. The Pershing could be available in five to nine weeks, depending on the trajectory, and would cost $5,000,000 to $10,000,000, excluding transportation. The Army preferred not to use the Pershing because of the effect on their in-house Pershing program. The Redstone could lift [redacted] to 92 kilometers altitude on the tactical trajectory. It would not have a warhead destruct capability, but command arm and fire could be provided. It could be ready in approximately seven weeks after authorization to proceed, and there would be very little impact on Army programs. The Hercules could lift [redacted] to altitudes as high as 95 kilometers with high accuracy, and it could be ready to fire within a month. It did not have a command arm signal, but did have command fire and destruct. In parallel with these considerations, Sandia started down the path of what eventually became known as the Strypi, a ballistic (unguided) missile that could lift [redacted] smaller systems to some of the lower altitudes being considered. The Strypi used an XM-33 rocket engine that had already been used for some of the instrument packages during the operation.

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The Kingfish argument now became real. It was quickly determined that it was feasible to change the trajectory of the Thor and that most of the experiments could be performed with a detonation point some 250 miles southwest of Johnston Island. However, such a change increased the operational problems greatly. The McMillan Committee eventually compromised on the trajectory, selecting a [redacted] burst altitude and thereby avoiding the possible eyeburn hazard in Hawaii. Sandia started down the path of preparing nearly every possible warhead for every practical missile. While all of this was going on a large part of the testing organization had scattered to the four winds, since there was no definite plan for future high-altitude shots and no possibility of any shots soon.

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On August 1 Kennedy accepted a Russian proposal for test ban monitoring by national means in all environments, but noted that this would require international supervision and on-site inspection. His discussion of the meaning of international supervision made it difficult to distinguish between that and international systems for monitoring. On August 6 the Soviets rejected his terms, having resumed testing on August 5.

By mid-August appropriate decisions had been made, and the system had started to settle down. Major reliance would still be on the Thors launched from two pads, but the Nike-Hercules and the Strypi would serve as backup systems for Bluegill. The Hercules and Strypi were also possible delivery systems for the small shots still being discussed. Starbird notified the testing organization to be prepared to test on one month's notice.

In the second half of August the tentative agreement on the burst altitude of Kingfish came unstuck. The McMillan Committee did not wish to accept [redacted] for the altitude [redacted] shot because the air density was still too high to allow the desired x-ray propagation. However, the trajectory had to be picked soon or Douglas could not put it into the Thor missile in time for the shot. Other questions arose for the small shots. The first two of these at [redacted] altitude presented no problem; however, the third, proposed at [redacted] posed a potential eyeburn problem in Hawaii, but it was not clear that the very small fireball to be produced would create a serious eyeburn hazard. By mid-August two Thor pads were being prepared for the larger shots, construction of two Hercules pads was underway as backup for Bluegill (the pads being equipped so that if one missile did not operate, the second could be launched immediately), and the XM-33 missile (Strypi) was in the final stages of development. Between the Hercules and the Strypi the small shots could be taken care of, but DASA had not yet decided that those shots

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should be done. It appeared that the Bluegill Thor could be ready September 20; the Bluegill Hercules by September 25; the Urraca Thor on October 2; a second Hercules or first Strypi by October 5; a third low-yield shot by October 15; and then the Kingfish Thor on October 17.

In late August, recogni[redacted] DDR&E had changed the proposed Kingfish yield [redacted] and added some small shots, namely Sideshow, [redacted] Tightrope, [redacted] at the highest altitude available [redacted], [redacted] at the highest altitude available to the Strypi [redacted]

However, the effects of Starfish on satellites and the Van Allen belts were now becoming well known. Scoville told Gerry Johnson that he thought the DOD was irresponsible in proposing any more high-altitude detonations. The problem came to a head at an NSC meeting on September 5. By now the Soviets were well into their second atmospheric test operation since the 1958-1961 moratorium, and some of the results were available. The McMillan Committee had done their job well in outlining the technical needs for Bluegill and Starfish and the kind of problems that could be answered by the small-yield detonations. Starfish Prime had raised a number of questions that could be investigated using data from these small shots. Furthermore, the shots could be interspersed with the Thor shots without prolonging the series. At the NSC meeting one of the small shots was deleted, but, perhaps more important, Urraca was thrown out by the President, both because of its possible effect on satellites and because the President really did not wish to develop another method of testing. His objective was to prevent testing, not to help it. In addition to these decisions, Kingfish was left floating because of the worry that at such a large yield, it too, would cause satellite difficulties. Thus, of the old high-altitude schedule there was only Bluegill to finish, but there were three new shots.

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During September th[redacted] continued. To make some progress, a tentative burst position [redacted] at 180 kilometers range on a bearing of 210 degrees from Johnston was chosen by WET with the concurrence of Ogle. However, this position was still not satisfactory to the McMillan Committee. In mid-September the situation was that if the intended yield was changed after September 25 there was no time to rebuild the pod instrumentation before the end of the series. The possible positions for [redacted] altitude on the old Thor trajectory, or [redacted] kilometers on the new trajectory mentioned above, or [redacted] altitude at 80 kilometers range on a bearing of 190 degrees. The first possibility, [redacted] could be done earliest, was satisfactory from the point of view of instrumentation, and presented no eyeburn hazard, but it was the least desirable experiment from the point of view of the McMillan Committee. The second position involved flying the Thor on a trajectory not previously used, but Douglas thought it would probably work. However, detonation at that altitude might lead to satellite damage. Ogle simply would not agree to the third possibility [redacted] because of the possible eyeburn problem in Hawaii.

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During the third week of August Ogle went to Washington and settled the problem with the McMillan Committee. Kingfish would be at [redacted] altitude. Of course, later in the week the decision came unstuck again. On September 20 McNamara agreed that preparations would continue for the [redacted] but he also requested that work be carried forward in parallel to allow the [redacted] shot. Starbird requested still another review of this possibility. Ogle's answer was that "There's a limit beyond which human flesh cannot endure," but he went on to point out that the latest date at which the Field Command Test Unit could adjust the small rocket trajectory's instrumentation and pod instrumentation for Kingfish was the previous Monday, September 17, and that any changes from now on

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By September 1 the Thor pad construction, the Hercules preparations, and the Strypi preparations were sufficiently advanced to plan resumption of high-altitude testing, starting with Bluegill on September 23. However, the September 5 NSC meeting changed that. The third U.S. manned orbital space flight with astronaut Walter Schirra aboard was planned for September. Starfish results indicated some possible hazard, and, furthermore, the DAMP ship was required for his recovery. Thus, the NSC decided that no high-altitude shot would be fired before the next Mercury shot (MA-8, scheduled for September 25), and the last Dominic event would be November 1 or earlier. As a result of these decisions, the September high-altitude schedule showed Bluegill on October 1, Tightrope on October 9, Checkmate on October 15, and Kingfish on October 27. Various certification shots of the Strypi and the Hercules would also have to be done. Kiley immediately requested that Bluegill be slipped to October 4, since his optically instrumented KC-135 aircraft could not arrive until then, but the schedule held for the time being. By mid-September there were already rumors that the Mercury launch would be delayed [redacted]

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On September 10 Tightrope was planned [redacted] altitude and Checkmate [redacted] By September 18 DASA had convinced the Task Force that the DAMP ship was absolutely essential to their Bluegill experiments. However, as was noted before, the DAMP ship was also required for the MA-8 recovery exercise, which was to be done some 1,600 miles from Johnston Island. Consequently, in order to avoid repeated short delays, Bluegill Double Prime was rescheduled to be MA-8 plus 11 days, with the high probability that MA-8 would meet its scheduled September 28 date. This decision allowed the pace to slow a little on the island, and by September 24 the Mercury 8 schedule had slipped to October 3.

With the inclusion of the Nike-Hercules in the series as a weapon carrier, safety studies of its fusing and firing systems promptly began. After a safety meeting on September 11 Starbird asked for information on a number of other subjects, including the probabilities of certain types of malfunctions such as burning on the pad when launch is attempted, malfunction during the booster thrust, malfunction after separation, probability of success of the barometer backup, probability of firing without the fire signal being sent, etc. He was also somewhat unhappy at the concept of launching a second Nike immediately if the first one failed. Mustin immediately discussed the problems with Bill Carter of Redstone Arsenal, who offered a number of possible changes, but recommended none. The Hercules was designed for comparatively small warheads; [redacted] Starbird therefore asked Rod Ray to work out with Carter some positive guarantee that the Bluegill alternate could not fire below an altitude of 60,000 feet. Eventually a command arm circuit was installed to take care of this problem.

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The Strypi's firing system was the same as used on the Thor, and there was no particular question about its characteristics; however, certification was still necessary. The first Strypi certification occurred on Johnston Island on September 22. The mock warhead reached an apogee [redacted] and impacted downrange at 225 kilometers. The flight was fully acceptable for the Checkmate shot.

The finality of Urraca being deleted from the schedule had so discouraged LASL that Hoerlin had concluded not to fly his highly instrumented KC-135. Furthermore, Kiley was having problems getting his KC-135s back to the shot and was thinking very

Withheld Under 5 U.S.C. 552(b)(1) 3(a)(6) carefully about the necessary optical coverage. On September 14 he discussed this problem with the Chief of DASA, who then formally requested that LASL operate its optical aircraft on all subsequent high-altitude shots, noting that it had some unique instrumentation and that it provided backup in the event a DOD aircraft should abort. After some discussion Hoerlin agreed.

The MA-8 mission on October 3 was a success, and by October 5 the Bluegill Thor launch was scheduled for the night of October 14, assuming arrival of the DAMP ship at Johnston. (That ship had had to turn back to Midway to off-load a seriously ill individual and was scheduled to arrive at Johnston on the 14th.)

On September 30 another Strypi certification was fired successfully, and on October 1 the second try of the Nike-Hercules Bluegill certification round was fired successfuly. (On the first try a manually operated switch had not been turned on to allow the EG&G timer signal to initiate launch.)

The DAMP ship arrived as planned, and on October 14 the Task Force was ready to do Bluegill. However, unfavorable weather forced a delay to the night of the 15th. Shortly after launch on the night of the 15th the Thor again failed in flight, and the warhead and missile were destroyed. This failure was apparently due to troubles in the guidance system. The Thor crews and, for that matter, everyone else were tremendously dejected. Starbird effectively gave up on the Thor, suggesting that the Douglas representative sit to one side for a little while and contemplate the situation. However, there was no choice: the Task Force had to make the Bluegill attempt with a Hercules. The system stalled a little and then scheduled the next attempt for the night of the 22nd or 23rd, depending upon whether the Hercules or the Thor was picked.

In the meantime, Checkmate, [redacted] had been scheduled for the 19th. There was a complete and successful dry run the day of the 19th, and that night the Strypi lifted the device to the proper altitude and the shot was fired successfully, cheering up the organization appreciably.

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Checkmate was beautiful. It was first a green and blue ring with spikelike protrusions at the edge, surrounded by a blood-red auroral ring which faded in less than a minute. Auroral streamers to the north and south formed immediately. Pink streamers were still visible 30 minutes after the explosion. [redacted]

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After Checkmate the system began preparing for Bluegill using the Hercules. In the meantime the Thor situation was reviewed, and Starbird proposed that the pods might be causing some of the problems, since, of the nine pods flown, only five had been within the acceptable range. He also noted that if the Hercules was used, almost half of the instrumentation rockets would have to be fired prior to the launch; if the Hercules launch failed, these instrumentation rockets would be wasted, precluding some measurements on yet a later Bluegill. He also noted that Sandia had been asked to prepare a Strypi missile as the primary carrier for Kingfish. In spite of these problems the remaining shots would use Hercules and Strypi as the primary missiles, with the Thor as backup.

The confusion about carriers caused the experimental system great difficulty because the timing of the equipment depended upon the carrier used. However, alternate plans were made and the timing system was set up to handle any of the proposed carriers.

On October 21 a second Nike-Hercules Bluegill certification was fired, but it self-destructed nine seconds after lift-off. The failure apparently resulted from

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loss of beacon return signal to the missile track radar 2.9 seconds after launch. (The October 1 Bluegill Nike-Hercules certification had been satisfactory, as had the Tightrope Nike-Hercules certification on October 6.) There was some hope that this difficulty was caused by high RF background noise on the island, so plans were made to do a Tightrope Nike-Hercules certification the next day to gain assurance that the Hercules was actually all right. Starbird argued to delay Bluegill until the [redacted] device could be put on the Strypi. (The Strypi had worked successfully every time it had been fired.)

The next day, October 22, 1962, at 4:45 p.m., the Nike-Hercules certification failed again in exactly the same fashion. The Strypi was not ready to use for Bluegill and its basic position inaccuracy made it a very undesirable missile for that shot, so the decision was made to try the Thor again.

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For the next two days the Hercules people worked on their problems while Sandia tried to prepare for all variations of shots with the Strypi. Because of all of the options, failures, and backups, Ogle asked LASL how long it would take to prepare another one or two [redacted] warheads. On October 25 Max Roy replied that one or two additional warheads could be available 21 to 25 days after a firm requirement was given.

November 1 was getting close. The fourth attempt at Bluegill using a Thor took place just after midnight on October 26: it was finally successful. Starbird left Johnston at 4 a.m., and that afternoon the airdrop test Calamity was fired. The decision was immediately made to switch back to Thor for the Kingfish shot. By October 29 it appeared too difficult to do Tightrope on a Strypi. The tentative suggestion was to try a Hercules again the next day, and if it failed, request that Tightrope be canceled. After several days of study, there was a tentative conclusion that the RF environment was disturbing the Hercules tracking system required for control. Consequently, for the dry run all environmental RF was cut down to the absolute minimum necessary for the test.

On October 30, following the Housatonic airdrop in the morning, the Hercules Tightrope certification was tried again and it worked properly.

Kingfish was attempted in the evening of October 31, but the weather was bad and there were problems in the Thor engine-position monitoring circuit. Weather window after weather window went by and finally, on the last opportunity of the evening, a little past 2 a.m., the Kingfish device was lifted to altitude and fired, using up our last Thor, our last RV, and the last pods. The rest of the night was spent in celebration.

The Kingfish success left one shot to be fired, namely, Tightrope, which had been relegated to the Hercules. On November 1 the Task Force gave the Army Hercules personnel one more day to be ready for certification, but there were still troubles. On November 2, by turning off most of the RF on the island and putting in a new amplifier decoder in the missile track radar, a successful Hercules certification shot was fired. The Tightrope shot itself followed on the next evening at 9:30 p.m. with complete success, ending the operation at Johnston Island.

The summaries of the results of the high-altitude detonations of Operation Dominic as given in the "Quicklook" reports are contained as Appendices B through F. (Ed. note: As noted at the end of the Christmas Island airdrop section, we have chosen to include here only an abstract of the document referred to because the author had not completed editing at the time of his death.)

EPILOGUE

R. Ray, R. R. Brownlee, H. M. Peek, and D. R. Westervelt

As Ogle might have put it, we had to fuss a bit with the question: What should we do about Chapter V? The decision to proceed as we have was not casually taken. It was clear that the author's concept of that unwritten chapter involved "Lessons Learned," but to the further questions: From what? About what? and By whom?, we were not certain of answers in spite of our close association with the project. Eventually we realized that the best evidence on which to base a decision could be found in Bill's own record of concerns and actions during the 20-odd years following the events of this account. Those years brought their own lessons, and Ogle played a vital role in bringing them to the nation's attention.

Thus, instead of a Chapter V we offer an Epilogue. It is in the nature of epilogues to summarize related events that occur after the conclusion of a story, and we believe that this is appropriate here. The text, as Bill left it, certainly contains many lessons specifically for those who may be called upon in the future to respond to a similar national need. We will summarize those that seem most important to us, but also will leave many more for the reader to discover. It was Ogle's style to provide leadership by making the system* think. We offer our own observations in the same spirit, and must assume full responsibility for the result while acknowledging our debt to the author as leader, tutor, colleague, and friend.

It is our belief that, strictly speaking, the period of test resumption in 1961-62 ended before the most important moratorium-related lessons for the nation had emerged. These lessons, the most important of which we have tried to state below, were not self-evident, but had to be learned as evidence accumulated.

Without knowledge of certain events that followed the history presented here, even a thoughtful reader might be excused for reaching conclusions that are erroneous. It would be possible, for example, to read this account carefully and note that, after all, it was possible for the United States to resume useful testing underground only two weeks after the first Soviet explosion; and that although the Christmas Island phase was delayed for a number of months, most if not all of the weapons tested there performed admirably. At this point, a reader might be tempted to ask "What is the problem?" The problem is that these facts, by themselves, are incomplete and misleading. Ogle, who was very much involved in the events that followed, never hesitated to point this out; we therefore see it as our duty to attempt here to do so.

*Several times in the text, and now here, "the system" is referred to. It is a term of art that we think deserves definition. Usually when Ogle referred to "the system" he meant the entire community of doers: politicians, executive decision makers, scientists, military players, and other operators who make things happen. One of the characteristics of the system is that it is seldom static for long (at one time the McMillan Panel was not an active part of the system, but it soon became one). Another is that it consists of subsystems and interrelationships, understanding of which can sometimes prove useful (the President is unlikely to overrule the Joint Chiefs on a crucial issue). Outsiders sometimes try to enter the system by offering "every assistance short of actual help." They usually fail.

Test Readiness

With regard to its ability promptly to resume underground testing, the nation actually was somewhat better off late in 1961 than it would have been if the Soviets had ended the moratorium a year or more earlier. As is noted in Chapter II, after an initially severe slump in test-related activities, and a dispersion of test personnel, the laboratories had regrouped their test cadres and had begun to acquire physical assets such as those assembled by Livermore with future Plowshare activities in mind. Los Alamos followed a somewhat different path, but the result was the same. Consequently, the most essential laboratory personnel were available for duty in September 1961, as were many key personnel of the technical support contractors. Construction and operational support was a major problem, as Ogle makes clear, because those assets were almost entirely dispersed or put in mothballs, and the actual methodology of underground testing remained troublesome for some time after resumption because little homework on this had been tried or accomplished during the moratorium. The availability of a technical cadre proved to be crucial when the President decided on a quick response to the Soviets, but this came about not from contingency planning, but as a fortuitous result of other influences.

The lesson here was that a quick response to national testing needs is likely to be available only if essential people and physical assets are kept active during a testing hiatus, by engagement in closely related and clearly useful activities. Evidence that this lesson was easily forgotten emerged a decade and a half later during intragovernmental negotiations sparked by the Carter Administration's efforts to achieve a comprehensive test ban. A chain of events that need not be recounted here, but in which William Ogle played a central role, finally led to a Presidential decision that any CTB negotiated with the Soviets must be of limited duration (comparable, as it turned out, to the moratorium), and that during that period experiments at the NTS involving small nuclear yields must be permitted. It was believed that a program of this kind would benefit both the weapons design technology base, on which stockpile confidence depends, and also the readiness of the nation to resume full-scale underground testing when the CTB expired.

This was a conclusion of great importance, but it was reached in 1978 only after protracted and often heated internal debate. The permitted-experiment activity that finally was sanctioned would have had some of the same effects as the Rover, Pluto, Plowshare and other activities did in 1958-61. The lesson, though it was recalled only with the greatest difficulty, was in part that such activities are essential unless testing is permanently renounced.

In retrospect, it is clear that the events chronicled in Chapter IV were far more traumatic than those in Chapter III, precisely because no comparable programmatic protection of cadre and assets existed in the area of atmospheric and high altitude testing. Perhaps because it was more dramatic, the nation more easily remembered this experience for a while after the moratorium. In particular, it had not yet been forgotten in 1963, the year when the U.S. and the U.S.S.R. reached agreement on a partial or limited test ban treaty (LTBT) that prohibited all but underground tests.

During Senate hearings on the LTBT, the Joint Chiefs of Staff, who were more sensitive to this piece of history than most, insisted on (and the Kennedy Administration promised to establish) four so-called Safeguards. The third of these, Safeguard C, required that the nation maintain readiness promptly to resume testing in the atmosphere and other prohibited environments, should this be required for national security. In effect, this Safeguard was a concrete reflection of the lesson learned from the moratorium.

The LTBT was ratified, and the country at first supported the readiness

Safeguard with funding and effort; two of us (RRB, DRW) became deeply involved in the resulting activity at Ogle's request. As a result of experience gained during the latter part of Operation Dominic, the Air Force was tasked in a joint memorandum of understanding to provide several NC-135 aircraft, to be developed, modified, and maintained as improved diagnostic platforms; dedicated and modified B-52 drop aircraft and a fleet of samplers were activated as well. A special Wing at the Kirtland Air Force Base maintained an ability to perform test missions, reinforced periodically by realistic readiness exercises that involved many of the laboratory personnel who had participated in Dominic. Ogle continued to involve himself in this activity, at first directly and later in an advisory capacity, even after, for practical purposes, the readiness program expired in 1975 with the loss of Air Force support and deletion by President Ford of the word "promptly" from the Safeguard.

Thus, the immediate lesson of the moratorium, that the nation should expend money and effort to maintain readiness to test in the atmosphere (and at high altitudes), was overtaken by a second lesson based on the post-moratorium experience. As the apparent likelihood of test resumption in the prohibited environments receded, it became progressively more difficult and eventually impossible to maintain readiness to do so. Even staunch supporters of the original four Safeguards, such as the late Senator Henry M. Jackson, eventually turned their attention to more pressing matters, and competition with other programs for funding became impossible. Control of much of the real estate was transferred to other agencies, many of the physical assets became obsolete or fell victim to neglect, and the personnel with relevant experience became scarce and now have all but disappeared. Even the tenuous hold on Johnston Island as a base for test operations was and is in jeopardy. Military expertise in test operation procedures quickly began to disappear and now is nonexistent.

Two other LTBT Safeguards (strong weapons laboratories and a vigorous underground test program) have more or less survived, although both are threatened by recurring attempts to eliminate testing entirely. Safeguard D, the ability to monitor testing by other nations, currently (1985) is enjoying something of a resurgence after years of neglect. As Dr. Foster observes in his Foreword, the future as regards atmospheric test resumption is clouded. Should the requirement once again arise, the account provided by Ogle may prove to be the nation's most important readiness asset.

Stockpile Considerations

The evident success of the weapons tests performed during the Christmas Island phase of this account carries its own danger of misinterpretation. It would be easy to conclude from these results that testing was really unnecessary because the validation of moratorium designs demonstrated that it was possible to design weapons during a testing hiatus and confidently put them into the nation's stockpile. Only during the following sustained period of underground testing was it learned how wrong that conclusion would have been.

The lesson here has been restated many times, most recently in a September 1985 statement by Robert N. Thorn, Los Alamos National Laboratory Deputy Director, before the Special Panel on Arms Control and Disarmament of the House Committee on Armed Services:

> With resumed U.S. testing in the aftermath of the Moratorium, we discovered technical problems with several weapons systems. As a result of the Moratorium, we lost many people from the weapon program. If it had not ended when it did, we would have remained ignorant of stockpile problems and suffered further personnel attrition.

The experience of the moratorium and the surprises immediately after it ended, but most especially the dismaying results obtained later as underground testing continued into the 1960s (in all of which Ogle was personally involved), led him in August 1977 at a meeting of a senior scientific advisory panel, to express surprise at the apparent indifference of the military about the Carter Administration's CTB proposal. The remark, offered in the usual Ogle style, was instrumental in prompting the subsequent recognition by the Joint Chiefs of Staff, and eventually by the President himself, that a protracted CTB was not in the national interest. The conclusion reached at that time has survived, although the design laboratories, Los Alamos and Livermore, increasingly have compromised their position by agreeing to the adequacy of partial-yield tests both for primaries (during the Carter Administration) and for high-yield weapons (under the restrictions of the Nixon TTBT).

Had the moratorium not ended, it is now clear that by the mid-1960s a large fraction of the U.S. stockpile would have been in serious trouble, and without recourse to testing there would have been a major loss of confidence in some weapon systems and false confidence in the performance of others. The problem was stated clearly by Thorn:

> Our calculation of risks and benefits [from the Moratorium] was affected by our overconfidence, perhaps one could say arrogance, in the state of our knowledge of the weapon R&D program and weapon tests before and leading up to the Moratorium. Looking back, this is astounding.... The Moratorium would amply demonstrate that there was much we did not know, and experience later showed that we totally failed to recognise our ignorance at the time.... Ultimately we did certify some weapons that had not been tested, in the belief that our understanding and design codes were satisfactory. In some cases the weapons proved out in testing after the Moratorium, in others they did not. The key point here is that we went ahead and made these decisions, under the pressures of the time and our excessive belief in our theoretical understanding and design codes . . . I can only say now, with the benefit of considerable hindsight, that such reliance was (and probably would again be) an almost irresistible temptation in the absence of nuclear tests to prove out our theories and validate our design calculations.

The implied lesson apparently was learned better by Ogle than by many of his colleagues, who only recently came very close to repeating the errors of the distant past. Again, Thorn explains:

> A very recent experience shows that we still can make mistakes in spite of the great advances in our computers and experimental techniques. The case involved one of our most important new strategic systems. Safety requirements for this weapon were especially tight, as were the constraints placed on the delivery system for which it was being designed, and there were still other considerations that made this a particularly challenging assignment. In spite of these sometimes conflicting priorities, we were entirely confident that the weapon we designed would perform as required.
>
> After the design was completed and certified for production, another contingency was brought up that had not been duplicated in the test program up to that time. Most of the key participants judged that no further test was required in order to have high confidence in the weapon under all circumstances, but a few, mindful of past misadventures, convinced us we should do another test simulating the new conditions. When this test was done (after production had started) it failed dramatically. The weapon would fail under certain conditions that it very likely would encounter. Because we were able to do additional nuclear tests, we could confirm the performance of a replacement design expeditiously, and production was interrupted only briefly.

It was, in fact, William Ogle who first raised the question of the neccessity for an additional test of this weapon.

A most important conclusion, then, reinforced by the events of recent years, is that a nation that depends in a fundamental way on nuclear weapons for its security cannot safely dispense with nuclear weapon testing. This conclusion depends on another: that a competent nuclear weapon technology cannot be preserved indefinitely without a test program. We know now that nuclear weapon design was, and to a large degree still is, an empirical rather than an exact science. Weapons are not designed from "first principles." Although both calculational and laboratory techniques have improved dramatically since 1961, those responsible for certification of the performance of the weapons in the U.S. stockpile believe that they require the ultimate proof of a successful nuclear explosion. Thorn concludes his statement of his Laboratory's position in 1985 as follows:

> [Under a CTB] If a problem were detected with a stockpiled weapon. . .we would again be unable to determine its seriousness or validate proposed solutions with nuclear tests. . . .With a relatively small number of designs in the stockpile, usually intended to remain there for many years, a problem with a single design could have a serious impact on our nuclear deterrent. This problem is worsened, in my view, by the unforgiving nature of current nuclear weapon designs. . . .
>
> Despite this fact, the risk that we would come to rely too much on theory, codes, and non-nuclear tests during a moratorium is probably even greater today. Fewer of our designers remember the chastening experience of the Moratorium, and the years that followed, and because our calculational tools are more elaborate and refined, it is easier to believe that they truly simulate nature. Thus, we could again be led seriously astray without the ability to validate our calculations and designs from time to time. As time went by, we would probably be tempted to develop, certify, and stockpile untested weapons again.

The immediate post-moratorium period is replete with illustrations of the vital importance of testing the weapons on which the national security depends. The active role of William Ogle in making the system think about the issue and reach this conclusion leaves little room for doubt that he would wish this major lesson from the moratorium period and its sequelae to be repeated here.

Systems Testing and Realism

Two questions were repeatedly posed by Ogle: (1) Is the U.S. doing all it reasonably can do to achieve maximum confidence that operational nuclear weapon systems will perform as planned, if they have to be used? and (2) If not, shouldn't we change the procedures to do so?

No one U.S. organization is responsible for ensuring the performance of an entire nuclear weapon system. Instead, many organizations, including the DOE, the Military Services, the Joint Chiefs of Staff, and other DOD elements, separately contribute information about their functions that is used in formulating and developing policy and war plans. This approach makes it likely that not every aspect of a system will ever be fully understood until the entire system is actually used. Nuclear testing history includes several examples of such "interface" problems which were not found until either planning or execution of some end-to-end test of the entire operational system was accomplished.

Another aspect of current systems testing practice that Ogle considered a weakness is our inability or failure to comprehend and simulate the hostile effects of the system environment which may influence system performance.

Illustrative of his concern about such problems, we have become aware of a private communication upon which Ogle was working just a few days before his death. On the one hand, he wrote: "There is a tendency to try to think of what might be wrong with a system and then to argue that the test should be worked in some way to look for that problem. To me that illustrates a basic philosophical error. We are looking for the problems that we cannot imagine!" He also wrote: "All of these items have of course been considered in going into stockpile, and in general, the judgment made that they cannot produce serious effects. But, can any of them lead to minor glitches that nevertheless will affect overall system performance?"

Ogle believed, and so do we, that an understanding of these problems should be an urgent task for those responsibile for nuclear weapons systems lest estimates of system performance are revealed, in a time of crisis, to be dangerously optimistic.

Safety in Testing

In these discussions of specific lessons for those who may have to repeat the 1961 experience, we take the position, as Bill Ogle did, that everything we talk about we talk about in the context of peacetime. In war much higher risks are accepted than when nations are at peace, because war is an incredibly high risk business. But in peacetime, although we are developing and testing the tools of war, the entire system must accept the values and the constraints of a society at peace.

Over the years, the Department of Energy and the national nuclear weapons laboratories, in concert with the Department of Defense, have evolved methods of testing and proving physics principles, design concepts, and weapon configurations in a field laboratory setting which has also provided a high degree of safety for the (world) public and for the members of the test organization.

In the later years of atmospheric testing, there were efforts made to conduct tests of military systems under realistic operational conditions. In fact, however, as the author makes clear in several places in Chapter IV, no nuclear weapon system in its standard military configuration provides adequate built-in safety for realistic, full-scale testing in peacetime. This assertion, which we are willing to state as fact, should provide ample challenge for test program and military planners, some of whom recognize the need for realistic operational systems tests.

Operational Trade-Offs

Each test operation, and in fact, each test involves a number of compromises. We have already alluded to the need for compromise in the testing of operational systems. Public health and safety, and the safety of test participants, have invariably led to hardware or procedural modifications that have just as invariably been resisted by the sponsors of the test. The author has given us several examples. We have discussed also the matter of political compromise. (Some would say scientific and technical compromise for political reasons.) In addition, though, we have important experience with compromise within the scientific organization itself.

A first category of compromise has to do with what we shall call test configuration. Each test event is conceived and justified to examine and investigate one or a few principles, concepts, or hypotheses. But once the individual test is approved for planning, it is viewed, properly, as an experimental opportunity. This may lead to modifications of the original test plan, and even of the test device itself, to accommodate additional experiments, and thus to make the test event more productive of useful data. The author has discussed a classic example of this process in the high-altitude series of the Dominic operation, when the McMillan Panel, for cogent scientific reasons, prevailed upon the test organization to make several important configuration changes at quite late times in the preparation for the series. The lesson here is that the technical managers of the program must be prepared for such eventuality, but must develop and enforce a discipline of their own, to assure that the primary test objectives are not unduly compromised and that accommodation of the needs of one test participant does not inadvertently harm the interests of another.

A related category of compromise comes during the operational or test execution phase. Not all test participants will have achieved the required high state of readiness at the same time. Nor will all find the same set of operational conditions optimum. Yet, typically, a delay in favor of one experimenter will lead to deteriorating readiness of another. The test organization management (read Scientific Deputy) must be thoroughly conversant with the purpose, experimental requirements and relative importance of each of the primary and add-on experiments, and must have the perception and courage to choose among them when that is called for.

Several aspects of these compromises are well illustrated by Ogle's account of the ASROC and Polaris systems tests of Dominic.

The DOE/Laboratory Role

Under current United States law, it is inconceivable that a full-scale nuclear weapons test program might be carried out except as a joint venture of the national laboratories and their federal sponsors (today DOE) and the military services (the DOD). Looking back over this history of some of the momentous years of the nuclear test program, it is well to consider the unique position of the Scientific Deputy Commander of the Joint Task Force (Ogle was the last incumbent of that position during atmospheric testing). In some future situation, the titles may be different, and the organization may have a different outward appearance, but the functional relationships are likely to be similar. It seems reasonable that if full-scale testing should again go outside the currently established test sites, the Secretary of Energy--with all of the technical and scientific resources of his department--will be expected to regenerate the equivalent of a scientific task group, a principal scientific advisor, and a technical support organization. The authorities, functions, and responsibilities of the Department of Energy relating to weapons testing derive from the Atomic Energy Act--the same act that established the AEC many years ago. The DOE is required "as a matter of continuing responsibility" to participate in the development of special safety studies, including those pertaining to nuclear detonations of whatever nature.

With history as our guide, we would expect to see weapons scientists of the DOE laboratories as advisors at all levels from the White House on down, and as active responsible agents in the execution of test plans. In years past, using our Pacific test experience as detailed in this work, this has been facilitated by the establishment of a Joint Task Force, reporting jointly to the AEC and the Joint Chiefs of Staff. The Task Force staff was integrated, with a senior AEC scientist serving as the Deputy Commander. This Deputy Commander had a direct reporting channel to the Atomic Energy Commission. We should note the political, operational, and scientific roles of the Scientific Deputy, and understand the importance of a good match between that person and the one who may be named as Task Force Commander.

The Political Environment

At the national political level, too, we should consider the vital roles of the Scientific Deputy and other scientists of the test community, for the political imperatives and the scientific realities are often if not on a collision course, at least on divergent paths. Timely and substantive interaction between responsible scientists and responsible politicians is both essential and inevitable; yet neither is entirely comfortable in the other's domain (or at least, if he is, he is probably suspect in his own house).

Perhaps the classic example of this imperative has to do with the timing of a resumption of testing after a moratorium, or even after a simple lapse in test activity. Historically, on each such occasion, there have been influences thought by the scientists to be extraneous, but considered by the national leadership to be compelling. On occasion, a test or a resumption of testing has been delayed to allow a political process to proceed without distraction. This can be frustrating to the test organization; but when the reverse occurs, that is, when overriding political imperatives influence the test organization to proceed ahead of its own schedule, the price may be very high indeed, not just in the morale of the scientific organization, but in the quality and validity of urgently needed test results. It is idle to lament this conflict, and dangerous to pretend that it does not exist. The key scientists in the test community must actively seek to inform the decision makers in Washington of the realities of testing, and must seek also to understand and find accommodation with the political world in which they live.

In a time of urgency approaching national emergency, regardless of the scientific imperatives, that which is inconsistent with the then-current national political initiatives *probably will not occur*. The corollary--just as true--is that political imperatives can give sufficient impetus to unsound technical initiatives to bring them to life even over the objections of responsible scientists.

In our time, perhaps no other scientific activity has had such an immediate and volatile interaction with national politics. For the knowledgeable scientist to participate in the political process without himself becoming politicized, or being viewed as a special interest lobbyist, is difficult; but it must be done.

Conclusion

We conclude this Epilogue with a final quote from the recent statement by the Los Alamos Deputy Director:

> In early 1962 President Kennedy, reflecting on the experience of the Moratorium, . . . said that in the future the US would find acceptable only written agreements which provided for an adequate inspection system in regard to preparations as well as testing. He emphasized that "this must be a fully effective treaty. We know enough now about broken negotiations, secret preparations and the advantages gained from a long test series never to offer again an uninspected moratorium. Some may urge us to try it again, keeping our preparations to test in a constant state of readiness. But in actual practice, particularly in a society of free choice, we cannot keep top flight scientists concentrating on the preparation of an experiment which may or may not take place on an uncertain date in the undefined future. Nor can large technical laboratories be kept fully alert on a standby basis waiting for some other nation to break an agreement. This is not merely difficult or inconvenient--we have explored this alternative thoroughly, and found it impossible of execution."

It appears that this fundamental lesson must be relearned often by the nation's ever-changing leadership. If it is forgotten, the other lessons become meaningless. It is our hope and belief that Ogle's account will serve the nation well. Certainly that was his intention.

APPENDIX A

A QUICK AND CURSORY SUMMARY OF THE CHRISTMAS ISLAND PORTION OF OPERATION DOMINIC 1962

SUMMARY

The Christmas Island portion of Operation Dominic consisted of the firing by air drop of twenty-four nuclear devices to satisfy the large yield weapon development testing needs of the Atomic Energy Commission. Twelve LRL and twelve LASL devices were fired.

The total yield of each device was deduced from fireball diameter vs. time and from bhangmeter data, and the fission yield by radiochemical analysis of bomb debris. The time intervals between stages were measured by electromagnetic and optical detectors. To check on the feasibility of an all-airborne measurement system, fireball cameras, time interval detectors, and distance measuring equipment were also operated from aircraft.

The Department of Defense conducted a number of effects measurements in conjunction with the AEC tests. Eyeburn studies, radar transmission studies, and close-in thermal radiation measurements were among the more prominent.

Weapons put into the stockpile during the test moratorium were tested and operated as designed. [redacted]

There was no appreciable fallout detected either on Christmas Island or any of the surrounding islands, and there was no damage from water waves. Damage from thermal radiation was very slight, and blast damage was generally minor, being limited for the most part to broken glass and studding and loosened panels.

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I. INTRODUCTION

During Operation Dominic some twenty-four devices were air dropped over the ocean near the southeast arm of Christmas Island. The main objectives of the tests were:

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APPENDIX B

STARFISH

General Summary of Results

Unfortunately, difficulties in pod stabilization and positioning seriously degraded the acquisition of data on the direct effects of x-rays on materials. Some of the material samples and indenter gauges were subject to the direct x-ray flux and the data are being analyzed; these should yield some useful x-ray effects information.

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1.3 (a) (6)
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Exemptions 1 & 3

An interesting side effect was that the Royal New Zealand Air Force was aided in antisubmarine maneuvers by the light from the bomb. The next paragraph is an eyewitness report of the detonation by Major C. X. McHugh, who was on Kwajalein; the paragraph following that is an eyewitness report from Johnston Island.

At Kwajalein, 1400 miles to the west, a dense overcast extended the length of the eastern horizon to a height of 5 to 8 degrees. At 0900 GMT, a brilliant white flash burned through the clouds, rapidly changing to an expanding green ball of irradiance extending into the clear sky above the overcast. From its surface extruded great white fingers, resembling cirro-stratus clouds, which rose to 40 degrees above the horizon in sweeping arcs turning downward toward the poles and disappearing in seconds to be replaced by spectacular concentric cirrus-like rings moving out from the blast at tremendous initial velocity, finally stopping when the outermost ring was 50 degrees overhead. They did not disappear, but persisted in a state of frozen stillness. All this occurred, I would judge, within 45 seconds. As the greenish light turned to purple and began to fade at the point of burst, a bright red glow began to develop on the horizon at a direction 50 degrees north of east and simultaneously 50 degrees south of east expanding inward and upward until the whole eastern sky was a dull, burning red semicircle 100 degrees north to south and halfway to the zenith obliterating some of the lesser stars. This condition, interspersed with tremendous white rainbows, (Ed. note: meaning unclear) persisted no less than seven minutes.

At zero time at Johnston, a white flash occurred, but as soon as one could remove his goggles, no intense light was present. A second after shot time, a mottled red disc was observed directly overhead and covered the sky down to about 45 degrees from the zenith. Generally, the red mottled region was more intense on the eastern portions. Along the magnetic north-south line through the burst, a white-yellow streak extended and grew to the north from near zenith. The width of the white-streaked region grew from a few degrees at a few seconds to about 5-10 degrees in 30 seconds. Growth of the auroral region to the north was by addition of new lines developing from west to east. The white-yellow auroral streamers receded upward from the horizon to the north and grew to the south and at about two minutes, the white-yellow bands were still about 10 degrees wide and extended mainly from near zenith to the south. By about two minutes, the red disc region had completed disappearance in the west and was rapidly fading on the eastern portion of the overhead disc. At 400 seconds, essentially all major visible phenomena had disappeared except for possibly some faint red glow along the north-south line and on the horizon to the north. No sounds were heard at Johnston Island that could be definitely attributed to the detonation.

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APPENDIX C

CHECKMATE

General Summary of Results

At Johnston Island, Checkmate observers first saw a green and blue circular region with spikelike protrusions from its outer edge. This region was surrounded by a blood-red ring which faded in less than a minute. Streamers oriented magnetic north-south formed almost immediately and gradually straightened out the initial circular patch. The blue-green streamers and numerous pink striations eventually extended to about a 50-degree elevation to the north and 10 degrees away from the burst to the south. The blue-green streamers faded out at about plus three minutes, leaving pink streamers which gradually faded, but were still visible at plus 30 minutes. A faint red patch was seen for a few minutes to the north, below and beyond the streamers.

At Samoa, observers saw a conical-shaped bright white flash originating some 45 degrees above the horizon and terminating at the southern magnetic conjugate point. The white color faded in a few seconds leaving an orange glow at the conjugate point which then faded completely by about H plus 1 minute.

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APPENDIX D

BLUEGILL

General Summary of Results

Observers at Johnston noted a brilliant white flash and a noticeable thermal pulse that was readily felt on the bare skin. At +10 seconds the burst appeared to be a slightly distorted, bright, moonlike-sphere with a clouded inner portion. As the sphere expanded its outer edges resembled a transparent shock wave. Inside was a denser, irregular, luminescent core which first appeared bright yellow and gradually became colored with subdued hues of green, pink, and violet. The central material moved to the surface of the sphere, forming a toroid whose center glowed with a purple fluorescence. Blue-purple streamers formed with the evolution of the toroid, extending about 15-20 degrees from the toroid, north and south along the magnetic field. The streamers, which appeared to come to a focal point in the south and to form a fan toward the north, lasted about three minutes, gradually disappearing. The toroid filled with luminescent wispy material and took on the form of a large, fairly uniform, glowing cloud. At +10 minutes, the cloud was about 120 degrees in diameter and its glow easily permitted resolving the dial of a watch. The cloud glow slowly died away, being still visible at +30 minutes, but no longer apparent by about +1 hour.

From Samoa, observers reported a narrow band whose color changed from bright pink at the northern magnetic horizon to green about 30 degrees above the horizon. The width of the band was about one finger at an arms length (Ed. note: about 1 1/2 degrees), spreading to three fingers, or about 5 degrees, after 3 minutes. The band faded to a dull pink with the green disappearing. By +10 minutes the width was constant at about 5 degrees, but the color had faded. The band was still visible at +20 minutes.

From high-speed photographic records, the following more detailed picture of the fireball and debris motion can be built up.

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[redacted]

The 2,500-foot and 6,000-foot range pods carried aloft on the Thor have been recovered in good condition. The middle pod impacted abnormally and suffered moderate structural damage; its instrumentation was in fair condition. Good tracks were obtained and orientations appear to have been correct on all pods. Quantitative data from the pod experiments are not yet available; however, it appears that the pod experiments on Bluegill were more successful than those on either Starfish or Kingfish.

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Monkey and rabbit eyeburn data were obtained in the four C-118 aircraft and on Johnston as part of the DASA retinal burn study. Two inadvertent human eye exposures occurred, resulting in bilateral foveal burns. Neither person suffered any discomfort, but both have lost significant amounts of their central vision. These case histories are being followed by project personnel.

APPENDIX E

KINGFISH

General Summary of Results

As seen from Johnston Island, a few seconds after burst there was a ring with a nearly transparent outer edge and an inner luminous circular region containing an irregular cloud-like mass. The outer edge quickly disappeared, leaving a luminous white-yellow region. Observers on the ground then saw what appeared to be two nonconcentric, circular areas moving rapidly northward. The two circles seemed identical in size, with one displaced magnetic north of the other. The north edge of the northern circle became increasingly irregular as spikes grew northward from it. At about +60 seconds, intense purple streamers had grown to the north, with several early green streaks. At times, there appeared to be rapid, twisting motion in the northward purple streamers. A purple glow region about 10 degrees above the northern horizon was separated by about 20 degrees elevation from the purple-green streamers and persisted until +10 minutes. The luminous circular regions straightened out into purplish, magnetic north-south striations by about a minute. To the magnetic south of the burst an oval, pale-green patch appeared early, persisted, and grew. This large pale-green patch south of, but near the burst point, was the dominant visible area after +5 minutes. This green area grew into an elliptical region with the long axis oriented east-west, and appeared to grow westward. At +10 minutes the oval extended about 30 degrees east-west and 20 degrees north-south. At about +20 minutes stars became visible through the green oval region. At +60 minutes the green area had lost its color, but had grown to be 120 degrees east-west and about 80 degrees north-south. At this time most of the light was emanating from areas close to the burst location. The dull gray region persisted for at least 30 hours after burst.

The event was first visible from Oahu as a bright flash of light on the southwest horizon. About 10 seconds later a great white to pink ball appeared to rise slowly out of the sea, preceded by a surrounding ring of red light. As the fireball rose above the horizon it appeared as a white sphere, somewhat egg-shaped, completely surrounded by a well-defined red ring. As it continued to rise the red ring diminished in brightness and the white ball elongated vertically, being asymmetric at the bottom. The cloud stablized at an elevation about 20 degrees above the horizon and flattened out as the red ring disappeared and the cloud faded. Eventually, the debris separated into two platters, one above the other, with their centers canted 15 degrees to the horizon, the lower end to the observers' left. The cloud was still easily visible at +7 minutes, but was no longer visible after about 9 minutes.

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Pod release and pod tracking appear to be satisfactory. The near pod was recovered in excellent condition, and superficial examination indicated that all instruments functioned and recorded data. Pod orientation appeared satisfactory. The middle pod was recovered, with the backplate and major portion of the flare and tracking antenna portion of the nose missing. The indenter gauge on this pod was recovered. The pod appeared to have been within 20 degrees of its desired orientation at burst. The third pod was recovered, but the backplate and almost all experiments were lost.

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APPENDIX F

TIGHTROPE

General Summary of Results

On Johnston Island, the Tightrope detonation was accompanied by an intense bright flash. Even with high-density goggles, the fireball was too bright for direct observation during the first few seconds. A distinct prompt thermal pulse was noticable on bare skin. The initial bright yellow-orange disc rapidly evolved into a doughnut shape with purple tinges. By about 60 seconds the torus was well-formed, had sharp edges, and was purple in color. The torus soon became purple throughout. By about 200 seconds, the torus had become crownlike in appearance and had fringes extending outward from the outside edge. The inner edge remained uniform and circular. By 240 seconds, the purple color of the torus became less intense and the slowly deforming torus was cloudlike in appearance. In a few minutes the residue appeared as a glowing purple cloud that was still faintly visible at +10 minutes. The cloud slowly moved north until it was no longer visible.

From Hawaii, a short sharp flash of white light was visible on the horizon, lasting less than 2 seconds. No other evidence of the detonation was detectable.

No observable effects were seen at Tutuila, although the weather was reported clear.

The experimental effort on the Tightrope event was greatly reduced from that on previous high-altitude events. The lower altitude of the detonation, as predicted, did not provide the widespread disturbances and effects seen in earlier Dominic events.

In general, the phenomena noted and the effects measured were in accord with predictions. Visible effects were confined generally to the Johnston Island danger area, some 320 miles in diameter.

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GLOSSARY

ABM	Antiballistic Missile
ABMA	Army Ballistic Missile Agency
ACDA	Arms Control and Disarmament Agency
ADC	Air Defense Command
AEC	U.S. Atomic Energy Commission
AFB	Air Force Base
AFBMD	Air Force Ballistic Missile Division of ARDC
AFCRL	Air Force Cambridge Research Laboratory
AFLC	Air Force Logistics Command
AFOAT	Air Force Office for Atomic Energy
AFSC	Air Force Systems Command
AFSWC	Air Force Special Weapons Center
AFSWP	Armed Forces Special Weapons Project
AFTAC	Air Force Technical Applications Center
AGC	Amphibious Force Flagship
AICBM	Anti-Intercontinental Ballistic Missile
ALOO	Albuquerque Operations Office of U.S. AEC
AME	Angle Measuring Equipment
AMR	Atlantic Missile Range
AOC	Air Operations Center
ARDC	Air Research and Development Command, predecessor of USAF Systems Command
ARPA	Advanced Research Projects Agency
ASD	Air Force Aeronautical Systems Division
ASROC	Antisubmarine Rocket
ASWT	Advanced System for Weapons Test
AVM	Guided Missile Ship
AWRE	(U.K.) Atomic Weapons Research Establishment
BLICOS	Balloon Interim Capability in Outer Space
BREN	Bare Reactor Experiment, Nevada
BSD	Ballistic Systems Division
CIA	Central Intelligence Agency
CINCPAC	Commander-in-Chief, Pacific
CINCPACAF	Commander-in-chief, Pacific Air Forces
CINCPACFLT	Commander-in-chief, Pacific Fleet
CJTF	Commander, Joint Task Force
CMB	Chemistry and Metallurgy Division of LASL
CNO	Chief of Naval Operations
CONUS	Continental United States
CP	Control Point
CSAF	Chief of Staff, Air Force
CTB	Complete Test Ban, later used for Comprehensive Test Ban
CTG	Commander, Task Group
CTO	Continental Test Organization of DASA
CVE	Aircraft Carrier, Escort
CVS	Air-Sea Warfare support aircraft carrier
DAMP	Downrange Antimissile Measurement Project
DASA	Defense Atomic Support Agency, successor to AFSWP
DDR&E	Director, Defense Research and Engineering
DOD	Department of Defense
DOE	Department of Energy, successor to ERDA

DMA	Division of Military Applications of the U.S. AEC
DME	Distance Measuring Equipment
DNA	Defense Nuclear Agency, successor to DASA
DPNE	Division of Peaceful Nuclear Explosives of U.S. AEC
ECM	Electronic Countermeasures
EDT	Eastern Daylight Time
EG&G	Edgerton, Germeshausen, and Grier
ELF	Extra Low Frequency
EM	Electromagnetic
EMP	Electromagnetic Pulse
EOD	Emergency Ordnance Disposal
EPG	Eniwetok Proving Ground
ERDA	Energy Research and Development Administration, successor to AEC
FAA	Federal Aviation Administration
FM/AM	Frequency Modulated/Amplitude Modulated
FRC	Federal Radiation Council
FUFO	Full-Fusing Option
FY	Fiscal Year
GAC	General Advisory Committee
GMD	Ground Meteorological Device; for angle tracking of a device transmitting weather data
GMT	Greenwich Mean Time
H&N	Holmes & Narver
HE	High Explosive
HF	High Frequency
HQ	Headquarters
HRT	High Resolution Telemetry
IADA	International Atomic Development Authority
IBM	International Business Machines
ICBM	Intercontinental ballistic missile
ICOS	Interim Capability in Outer Space
IRBM	Intermediate Range Ballistic Missile
JCAE	Joint Committee on Atomic Energy (of U.S. Congress)
J.I.	Johnston Island; later coral filling created other islands and J.I. became J.A. for Johnston Atoll
JOC	Joint Operations Center
JOWOG	Joint (U.S./U.K.) Working Group
JPL	Jet Propulsion Laboratory
JTF	Joint Task Force
kt	kiloton
LASL	Los Alamos Scientific Laboratory
LCM	Landing Craft, Mechanized
LCU	Landing Craft, Utility
LLL	Lawrence Livermore Laboratory
LOF	Lowest Observed Frequency
LORAN	Long-Range Navigation
LRL	Lawrence Radiation Laboratory
LSD	Landing Ship, Dock
LTBT	Limited Test Ban Treaty
MATS	Military Air Transport Service
MIT	Massachusetts Institute of Technology
MLC	Military Liaison Committee
MOF	Maximum Observed Frequency

mR	milli-Roentgen
MSTS	Military Sea Transportion Service
Mt	Megatons
NAS	Naval Air Station
NASA	National Aeronautics and Space Administration
NATO	North Atlantic Treaty Organization
NBS	National Bureau of Standards
NEL	Naval Electronics Laboratory
NOL	Naval Ordnance Laboratory
NPG	Nevada Proving Ground
NPM	Call sign for a U.S. Navy VLF radio station
NRDL	Navy Radiological Defense Laboratory
NRL	Naval Research Laboratory
NSC	National Security Council
NTS	Nevada Test Site
NTSO	Nevada Test Site Office of the ALOO Office of Test Operations
NUTEX	Nuclear Tactical Exercise
NVOO	Nevada Operations Office of U.S. AEC
OCB	Operations Coordinating Board
OFO	Office of Field Operations of AEC-ALOO
OMB	Office of Management and Budget (formerly Bureau of the Budget)
PACAF	Pacific Air Force
PMR	Pacific Missile Range
PNE	Peaceful (Uses of) Nuclear Explosions
PSAC	President's Science Advisory Committee
RAF	Royal Air Force
REECo	Reynolds Electric and Engineering Company
rem	Roentgens-equivalent-man, a measure of biological dose
R	Roentgen
R&D	Research & Development
RD	Restricted data
RF	Radio Frequency
RV	Re-entry Vehicle
SAC	Strategic Air Command
SALT	Strategic Arms Limitations Talks
SBAMA	San Bernardino Air Material Area
SC	Sandia Corporation
SCLL	Sandia Corporation-Livermore Laboratory
Shake	1 shake = 10^{-8} seconds
SHAPE	Supreme Headquarters Atlantic Powers Europe
SRI	Stanford Research Institute
SRP	Savannah River Plant of AEC
SSD	Space Systems Division
STL	Space Technology Laboratory
SUBROC	Submarine Rocket
SWC	Special Weapons Center (for AFSWC)
TAC	Tactical Air Command
TG	Task Group
TREES	Transient Radiation Effect on Electronics
TTB	Threshold Test Ban
TTBT	Threshold Test Ban Treaty
TWG	Technical Working Group

TWX	Name adopted by telecommunications industry for electrically transmitted telegraph message
U.N.	United Nations
U.S.A.	United States of America
U.S.S.	United States Ship
UCRL	University of California Radiation Laboratory at Livermore
UHF	Ultra High Frequency
USAF	United States Air Force
USDA	United States Disarmament Administration
USGS	United States Geological Survey
VHF	Very High Frequency
VIP	Very Important Person
VLF	Very Low Frequency
WET	Weapons Effects Test, an element of DASA
WRS	Weather Reconnaissance Service
WWG	Weapons Working Group

Note: A few acronyms or abbreviations that occur only once, or a very few times, in the text have not been listed in the Glossary owing to our inability to track down their meaning. These are: CCVE and OZ, which apparently imply some types of ocean-going vessels, and APSC and USAS, which -- from the text -- were organizations that controlled or operated aircraft.

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INDEX

We have chosen not to list as index entries those that occur essentially on every other page, e.g., LASL, LLL, LRL, AEC, DOD. The editors apologise for any confusion these omissions may cause the reader.

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